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REPORTS OF THE PROCEEDINGS

OF THE

GEOLOGICAL & POLYTECHNIC

SOCIETY

OF THE

WEST RIDING OF YORKSHIRE,

1850.

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1851.

PROCEEDINGS

OF THE

GEOLOGICAL & POLYTECHNIC SOCIETY

Of the West-Riding of Yorkshire,

AT THE FORTIETH MEETING, HELD IN THE LECTURE ROOM OF THE HALIFAX PHILOSOPHICAL AND LITERARY SOCIETY, ON THURSDAY, JUNE 6TH, 1850.

The Rev. WILLIAM TURNER, having been voted to the Chair, introduced the business of the meeting in an appropriate address, and regretted the unavoidable absence of the Venerable Archdeacon Musgrave, and John Waterhouse, Esq., F.R.S., the President of the Philosophical Society, both of whom took a warm interest in the proceedings of the Geological and Polytechnic Society of the West Riding of Yorkshire. He had long watched with pleasure its operations—and noticed the various important communications which had been brought before its meetings, and he was glad to find that two subjects of such great interest as the Electric Light and Dia-Magnetism would be brought before them that day, and especially the former of these communications. He believed it was the intention of Mr. Dresser to describe a new and economic process for producing this extraordinary phenomenon, which he considered a great desideratum; as, in too many scientific discoveries, however apparently beneficial in their object, the expense involved was such as to place them beyond the reach of most persons, and it consequently acted as a check to their ever becoming generally useful to the public at large, which

was the principal aim of scientific research. Upon the second subject, Dia-Magnetism, which might be considered in its infancy, much highly interesting and novel information might be expected.

The following gentlemen were proposed as Members of the Society :—

Dr. Paley, of Halifax.
Thomas Hodgson, Esq., of Halifax.
J. W. Child, Esq., of Halifax.
Mr. Thomas J. Pearsall, Agent to the Yorkshire Union of Mechanics' Institutes, Leeds.

The Chairman then called upon Mr. Dresser to read the first paper—

ON A NEW AND CHEAP VOLTAIC BATTERY FOR THE PRODUCTION OF THE ELECTRIC LIGHT. BY MR. CHRISTOPHER LEEFE DRESSER, OF LEEDS.*

Mr. Dresser described the different voltaic batteries previously in use, and the circumstances which led to the discovery of the suitability of gas carbon for voltaic purposes, and then proceeded with the following description of the peculiar substance used in his battery.

In the retorts used for the destructive distillation of coal to form carburetted hydrogen gas for illumination, after a certain period a deposit of a carbonaceous nature is formed on the iron, and, once commenced, proceeds with great rapidity, accumulating to such an extent as to close up part of the retort, and line the whole with a coating varying in thickness from that of writing paper to several inches. This carbonaceous matter, though a good conductor of heat, is not so good as the iron of the retort, consequently the process of distillation is impeded, and the waste of the heating power becomes at length so great as

* The author will be happy to furnish the carbon, or in any way aid any one who may be inclined to repeat his experiments.

to compel the disuse of the retort. In many instances the iron of the retort is destroyed for a considerable space, and its place supplied by the carbon long before it is necessary to destroy the retort. From the continuous thickening of the deposit, it is evident that the process of deposition once commenced by the iron is continued by the red hot carbon.

The property of red hot carbon to decompose hydrocarbons, was proved from the fact of a common broom having been accidentally thrown into one of the retorts, which, when charred by the heat, rapidly decomposed the gas, the carbon of which formed a singularly beautiful deposit on the slender stems of the broom, being deposited like branches of coral, hard, elastic, of considerable length, and very pure. The substance assumes a variety of forms and appearances, some of them very beautiful. The density and hardness varies also as considerably.

The form of the deposit appears to be either minute scales, like a section of a hollow sphere, or thin layers. The scales are deposited on each other till the whole takes the form of mammillar protuberances, sometimes of great metallic brilliancy, as specimen No. 1, having but little aggregation, and easily crumbled betwixt the fingers. This I believe to be pure carbon, though when heated sulphur is driven off, which appears to be mechanically mixed, and not chemically combined, for heat drives it off, and leaves the scales brilliant and unaltered in appearance.

Specimen No. 2 has the same form as No. 1, but more aggregated.

Specimen No. 3 has the same form, with considerable hardness.

Specimen No. 4 has the same type, but the mammillar protuberances are large, and apparently solid, but when exposed to a red heat for an hour, exhibits the same

laminar form as the other. This specimen is very beautiful and singular.

Specimen No. 5. The mammillar form appears lost. The carbon has great hardness, equal to black marble, and the fracture is stony without any laminæ.

Specimen No. 6. Another appearance is presented of great metallic brilliancy, density, and hardness.

Specimen No. 7 is the same form as deposited on the charred broom, in needle-shaped ramifications, and when magnified is like a string of beads, very hard, elastic, and composed of pure carbon; at least I have been unable to detect any foreign substance, and there is no appearance of sulphur when heated.

Specimens Nos. 3, 4, 5, and 6, contain iron in a metallic state, but varying considerably in different specimens. No. 5 contains the greatest amount.

These forms of carbon will bear the most violent heat with very little waste. In my crucible furnace, urged with a large circular fan, raised far beyond a forge fire, it suffers but little waste, and gives an intense heat. Formed into crucibles, by a lump being cut hollow in a lathe, it is most valuable, standing where all others fail. The forms of this carbon, as shown in Nos. 5 and 6, are what I prefer in the voltaic battery, and may be obtained at any gas works at the most trifling cost, in plates of large size, and sufficiently thin to use in the square cells.

The most convenient form, and involving the smallest cost, also preferable in many respects, is to cut the carbon into sticks of about $1\frac{1}{8}$ inch diameter, and 7 long, and use them with the nitric acid in round porous cells, the surrounding zinc cylinders being about three inches in diameter. The cutting of the carbon is accomplished with the greatest facility and certainty, by the machine of a marble cutter, at the small cost of $1\frac{1}{4}$d. each, and can easily be obtained

from 6 to 18 inches long, and consequently be used with much longer porous cells.

The substance has but little porosity, but I prefer having the plates or cylinders about two inches above the top of the porous cell, as they will then remain in action for hours without affecting the connexions. The only precautions necessary are, after use, to immerse the plates in boiling water for a few minutes to abstract the acid, also to dry them well before a fire or in a stove, immediately previous to use; if these precautions are neglected the acid will absorb moisture from the air, and be always damp and affect the connexions. Often when in haste I place them on the fire, and sometimes heat them red, which in nowise deteriorates, but rather improves their action.

Having used the same plates and cylinders for a long time, no deterioration of their conducting power, nor any decomposition or alteration, has been observed. The connexion is made by soldering a strip of copper to the zinc, and pressing this very strongly against the carbon with a clamp. The more firmly the copper is pressed against the carbon the better will be the connexion. The copper should always be made bright where the contact with the carbon is made, and the surface of the carbon rubbed very smooth. The other parts of the carbon plate or prism may be left as taken from the retort or saw, its roughness increasing its acting surface.

Comparing these plates with plates of platinum of equal size, I can detect no difference, and from their cheapness and durability they will make a valuable addition to our voltaic apparatus. With a battery of 90 plates the voltaic light was of the most splendid description, $1\frac{1}{4}$ inch long, and very voluminous. My garden and a large field were brilliantly illuminated. The trees presented a very singular appearance; the leaves looked white instead of green.

Small print was easily read at a considerable distance, without any reflector behind the light. The cost of a battery of 100 pairs will be about £4, whilst one of equal power with platinum negatives would be £60 or £70.

The powers of the battery were tested by many brilliant experiments—as the ignition of great lengths of thin wire, the combustion of metals, the electric light, and the dispersion of that light by the prism.

In the course of the discussion which followed, Mr. Pearsall, in an interesting speech, alluded to the great importance which might accrue to the public, and especially in saving the lives of mariners, if the adoption of this magnificent discovery were brought into use in lighthouses, or sent out in steam vessels on particular occasions, as foggy weather, &c., to guide ships on dangerous parts of the coast; and hoped that no argument of such a paltry or disgraceful character as expense would interfere to prevent so benevolent a project being carried out.

Mr. W. S. Ward supported the proposal of Mr. Pearsall, with additional suggestions as to its applicability, and the means of recognising the different lighthouse stations, by the *number* of lights employed, &c.; also on the comparative cost per hour of a light such as Mr. Dresser had exhibited, as weighed off by a galvanometer in the room; and showed the fallacy of supposing this light, however beautiful, would ever supersede the ordinary gas for shops, streets, &c.

ON SOME PHENOMENA OF DIA-MAGNETISM, WITH EXPERIMENTS. BY WILLIAM SYKES WARD, ESQ., OF LEEDS.

In the course of some experiments, in which I was desirous of exhibiting the principal phenomena of Dia-Magnetism,

described by Dr. Faraday in the transactions of the Royal Society, I was at first surprised at obtaining different results from those published; many metals which, according to Dr. Faraday, had appeared to take an equatorial position between the poles of the magnet, assumed the axial position as magnetic bodies. I soon observed that the nature or direction of the action upon many metals varied with the intensity of the magnetic force, and I found that such effects were in accordance with the observations of Professor Plucker. On this he appears to have correctly founded the hypothesis, that the dia-magnetic force increases more rapidly than the magnetic in relation to the power of the exciting magnet.

Not being aware how far Professor Plucker may have extended his researches to the phenomena of the magnetic and dia-magnetic forces developed in pure metals, I conceived that the inquiry, how far such forces might be co-existent in the same metal, might lead to interesting results. In pursuing such investigations other phenomena presented themselves to me; and as these in some respects depend upon the power of the magnet employed, I will describe the apparatus with which the experiments were conducted. My electro magnet is composed of a bar of soft iron $1\frac{1}{2}$ inches in diameter, 11 lbs. in weight, and about 20 inches in length, bent into a horse shoe form. It is covered in the usual manner with about 120 yards of insulated copper wire, No. 12, Birmingham wire guage, connected in one length. The poles of the magnet are about $3\frac{1}{2}$ inches apart, and are furnished with polar pieces, the distance of which can be varied from about one-tenth to two inches, and regulated by set screws. The polar pieces are tapered so as to present plane faces in the form of parallelograms, which are about 0.3 inches in breadth, and 0.2 inches in depth. I generally employ four pairs of

Grove's elements, of which the platina is $2\frac{1}{2}$ inches wide, and immersed about four inches in nitric acid, in good flat porous cells. The quantity of current which the four cells cause to circulate through the magnet is generally equal to about 130 grains of zinc dissolved in each cell per hour. The mode of suspension was by a fibre of silk suspended within a glass cylinder, placed on a kind of stool above the magnet, which is used in an upright position.

In making use of a small magnet I found it advantageous to experiment on correspondently small pieces of metal, of about the size and weight of a fourpenny piece, (or about five-eighths of an inch in diameter, and 30 grains in weight. I suspended such pieces of metal as were readily pierced, by means of a hole drilled through the upper edge, and through which a copper wire, one-hundredth part of an inch in diameter, was fastened, with a little hook at the other end of the wire, to attach it to a ring at the end of the silk fibre.

Such arrangement is abundantly sufficient for the exhibition of the effects I am about to describe, and indeed with it I first noticed several phenomena which had escaped my attention when using a much larger magnet.

Most of the experiments I am about to describe were well exhibited by another magnet, of which the core was only six-eighths of an inch in diameter, and two lbs. weight, of the horse shoe form, and having about 30 yards of No. 18 wire coiled upon it, and having similar polar pieces. This magnet was excited by three small pairs of Grove's battery, with circular cells of the ordinary construction, having platina strips half an inch broad and immersed about three inches in nitric acid.

In submitting the metals—gold, silver, lead, tin, zinc, and cadmium, to the action of the magnet when excited by an electric current of only moderate force, for instance, when

only one cell of the battery was employed, the current through the magnet being equal to about 30 grains of zinc per hour, or when the polar pieces were at a distance of not less than one inch, the last mentioned metals took the axial position, pointing in the same direction in which a piece of platina would have done. But when the battery power was increased, and the poles brought nearer together, instead of the disc of metal being more strongly attracted, it became less sensible to either attraction or repulsion, and was very sluggish in its motions, retaining for some time any position in which it was placed; and although it did not remain stationary, as mentioned by Dr. Faraday with regard to copper, yet it moved to the axial position very gradually, and without the oscillation it exhibited when the magnet was either more or less powerfully excited. But when the magnet was both well excited and the polar pieces brought within a quarter of an inch of each other, most of the before-mentioned metals took a decidedly equatorial position, and were repelled as dia-magnetics, the sluggishness nearly disappeared, and the disc vibrated about the equatorial position as antimony or bismuth would have done. I shall afterwards more particularly mention the order in which such changes appeared to take place with various metals.

As I experienced much difficulty in procuring specimens of metals perfectly free from any admixture of iron, I at first doubted whether the attraction of the metal did not depend on the presence of a minute portion of iron, and that the repulsion afterwards exhibited was due to the peculiar condition of the metal; and I found that in some specimens of the metals which readily exhibited the presence of a small quantity of iron by the ordinary chemical tests, the magnetic state was much more decided than with purer specimens. But as some of the metals were as free

from iron as I believe it possible to obtain them, I think the phenomena of the variable magnetic and dia-magnetic states of metals may be safely considered as being independent of the presence of iron or other constantly magnetic bodies.

In experimenting upon the metals—gold, silver, copper, lead, and zinc, my attention was particularly drawn to the phenomena described by Dr. Faraday, and by him called "revulsion," and I found that the form of small flat discs was a particularly convenient one for exhibiting these. Dr. Faraday noticed the phenomena of revulsion, particularly in copper; I, however, found that it is exhibited in nearly all metals which are less magnetic than platinum, or less dia-magnetic than antimony. I noticed that the direction of the revulsive motion is different in magnetic and in dia-magnetic metals. I adopt Dr. Faraday's term revulsion, and I call the revulsion repulsive when the disc subjected to experiment is repelled on breaking circuit; and attractive when the disc is attracted under the like circumstances. I also noticed that in the metals, whether pure or compound, which passed from the magnetic to the dia-magnetic state, the direction of the revulsion also changed. That such a body, exhibiting attraction as magnetic, was vigorously attracted through a small space on making battery contact; and as vigorously repelled on breaking the circuit. That when the same body, by increasing the battery power, or more conveniently by approximating the polar pieces, was made to pass into the dia-magnetic state, the directions of such revulsive motions changed—that on making contact the metal was repelled; and that on breaking contact it was attracted: I found motions were in some cases to be relied on, as pointing out the magnetic and dia-magnetic state of the metal, when the steady pointing of the piece axially

or equatorially could not be observed. The revulsive motion not being exhibited by the disc of metal when it had assumed a position either strictly axial or strictly equatorial, in order that the phenomena should be fully developed, I found it necessary to prevent the disc from assuming such positions. With metals tending to the axial position this was easily effected, by closing the polar pieces so that the disc could not pass to the axial position, being stopped by the polar pieces themselves; but in experimenting with some metals in which the revulsive action is only manifested in a slight degree,—for example, with palladium,—some care is needful in ascertaining whether revulsion takes place or not, for if the disc of metal be allowed to touch the polar pieces, the vibratory action of the magnet appears not unlikely to communicate itself to the disc, thus producing the appearance of revulsion; therefore, in such experiments I found it convenient to use a steady support, by which the disc under examination is prevented going to the axial position. Such a support is also very useful in preventing a metal, affected dia-magnetically, from taking the exact equatorial position; but when a support is used, the motions require to be observed very carefully, for the disc appears to be moved from the position in which it would hang when not affected by the magnetic or dia-magnetic force, and to move, as though revulsively, by the mere action of gravitation. But on carefully noticing the motion, it may be observed whether it be more rapid than that due to gravitation alone.

The changes from the magnetic to the dia-magnetic state are, perhaps, most decidedly exhibited by pure zinc or pure lead. When a very considerable magnetic power is applied to these metals, either by increasing the power of the battery, or by approximating the polar pieces, instead of attraction, as magnetic bodies, they show repul-

sion, as dia-magnetics; and I found that after this change of condition the phenomena of revulsion were exhibited as before, but in a reverse direction; and that when the magnetic force was sufficiently energetic, repulsion took place on making contact, followed by attraction on breaking contact. In experimenting upon lead and zinc, if the support be moved to one side, so that one side of the disc just touch the flat end of the polar piece, the disc is repelled on making contact, and is held at a small distance from the polar piece, whilst the current is continued; but on breaking contact it is briskly attracted by the polar piece. To produce the last mentioned effects in a decided manner, although the effect may be observed with one polar piece, yet it is advisable to have the two close together, as the magnetic force does not appear to be sufficiently concentrated in the one piece if the other be not approximated to it.

It might be supposed that as gold, silver, copper, lead, tin, zinc, and cadmium, take an axial position when the polar pieces are distant, and an equatorial one when they are close, that neutral points would be found at which neither attraction nor repulsion would take place. I found that this happened with lead; but with gold, and particularly with zinc, the disc was attracted when contact was made, but afterwards, during a few seconds, it slowly moved to the equatorial position; moving extremely sluggishly, in the manner particularly noticed by Dr. Faraday with regard to copper. But, although during this sluggish state, the disc appeared to be little affected by any directive force towards the axial or equatorial position, yet, on breaking battery contact, the revulsive force was most strongly developed. And it was very remarkable that if the contact were broken before the disc had assumed an equatorial position, the revulsion was from the nearest polar piece,

tending to make the disc revolve. But, on the contrary, if the circuit were broken after the disc had assumed an equatorial position, the whole disc was swung towards the polar piece to which it happened to be the nearest, and with sufficient force to move the whole disc as a pendulum through an arc of two or three degrees; so that although the magnetic and dia-magnetic forces seemed as if balanced or neutralised, yet the revulsive forces corresponding with these were strongly manifested in either direction, as the case might be. It appeared always necessary that some time (about one second) should elapse before the magnet induced the dia-magnetic state; but I did not, in any instance, observe the disc to be in a neutral state as regarded the revulsion.

The motion communicated to the disc by the attractive revulsion is, however, very different from that exhibited by the repulsive revulsion. The latter always tended to make the disc revolve on its centre of suspension, both the edges being repelled from the polar pieces to which they were nearest; but the attractive revulsion caused the entire disc to move towards the polar piece to which it was nearest, without exhibiting any tendency to make the disc revolve.

For the purpose of ascertaining in what order different metals were affected magnetically and dia-magnetically, I instituted the following experiments; in which I made use of the eleven pound magnet, excited by the four cells of Grove's battery, as before-mentioned. Platinum and palladium were procured from sources which I could not trace, and, as had been observed by Dr. Faraday, these metals took the axial position, with weak or strong powers, or with the polar pieces distant or close; and the revulsion, although by no means so strongly developed as with other metals, was repulsive on breaking contact. Antimony and bismuth were from common commercial specimens, and had been

cast after being melted in an iron ladle; nevertheless they were decidedly dia-magnetic, even with very slight powers, and with the polar pieces distant: thus negativing the supposed need for great care to insure perfect freedom from iron, at least as regards these metals. Mercury was also found dia-magnetic under any power which would at all affect it in this respect. It did not distinctly exhibit any revulsion. Of arsenic I had not such a specimen as enables me to speak decidedly. It appeared to be magnetic with the poles distant, dia-magnetic with the poles close. The structure was so decidedly crystalline that I could not ascertain whether the change of position was not due to the varying ratio of the magne-crystallic force. The four last mentioned metals did not show any decided revulsion when the circuit was broken, but if any took place it was that of attraction, but it was too slight to be relied on; for the dia-magnetic action so repelled the body from the adjacent pole, that when such action ceased, the disc under trial would swing as a pendulum, under the mere action of gravitation. On very careful examination, however, it appeared that the motion, under such circumstances, was somewhat quicker than was due to gravitation alone.

The specimen of zinc which I selected for the purpose of these experiments was reputed pure, having been prepared for use in Marsh's apparatus, and had not been hammered or rolled. It was much less magnetic than other specimens of the ordinary sheet zinc. The polar pieces being adjusted at a distance of three quarters of an inch, (0.75,) the piece of zinc was attracted when contact was first made, and afterwards appeared sluggishly to retain any position in which it might be placed, but, nevertheless, slowly moved to the equatorial position; that is, it appeared magnetic at the first contact, but afterwards gradually

became dia-magnetic. If the circuit were broken before the disc of zinc had decidedly assumed the equatorial position, the revulsion was strongly repulsive; but the circuit being broken after the disc had assumed the equatorial position, or nearly so, the disc was strongly attracted: and if the disc were rather nearer to one polar piece than the other, instead of the disc tending to revolve on its centre of suspension, as in the revulsion from the magnetic state, it was moved entirely toward the nearer polar piece, the whole mass being decidedly attracted.

Gold.—The specimen used was a quarter-guinea piece; this was decidedly less inclined to become magnetic than zinc, when the polar pieces were at 0.75.

The piece was magnetic at the first contact, afterwards very sluggish, and only slightly sensible to either attraction or repulsion. At 0.38, gold exhibited the like phenomena as zinc at 0.75, and also at shorter distances.

The like phenomena were exhibited by zinc, as the polar pieces were progressively brought nearer each other, as afterwards mentioned. The change from the magnetic to the dia-magnetic state, and also in the direction of the revulsive motion on breaking circuit, was most decidedly shown when the polar pieces were at a distance of 0.37.

I procured *Lead* by voltaic deposition from nitrate of lead, the metal thus obtained was redissolved in pure nitric acid, and again electrolitically deposited, then fused on glass, with the addition of a little tallow as a flux; the piece thus obtained appeared decidedly magnetic at 0.75; was very sluggish at 0.56: took a diagonal position, with the edges of the disc nearly touching the polar pieces, but still sensibly separated. With closer adjustments of the polar pieces, lead behaved as the zinc had done at greater distances.

Cadmium—(the purity of this was not tested, but it was

reputed to be pure)—continued to take the axial position when the polar pieces were approached to 0.29, but its motions then became very sluggish; and at 0.15, after being first attracted, took a decidedly equatorial position.

Tin—(this was deposited, electrolitically, from a solution of protochloride)—continued to tend towards the axial position, or to be attracted till the polar pieces were approached to within 0.15; the motions were then very sluggish, but exhibited signs of being attracted on breaking circuit.

Silver and Copper continued to be attracted, the polar pieces being at 0.15. The silver was prepared by reducing precipitated chloride of silver by carbonate of potash, in a small crucible of pipe clay. The copper was deposited from sulphate of copper by the electrotype process.

The polar pieces being closed to within 0.1, the zinc and gold were yet attracted at the first completion of the voltaic circuit, but afterwards appeared as though floating between the two surfaces: but lead at once took the equatorial position, without experiencing any attraction. Tin and cadmium were also repelled after the first contact. Copper and silver continued to be attracted.

The experiments with the polar pieces so close as 0.1, are not altogether to be relied on, as regards the revulsive movements, for the lateral motions hereafter mentioned took place so decidely as to render the other movements very difficult of observation.

One of the polar pieces being altogether removed, and the discs being hung at a distance of about a millimetre from the other polar piece, and the arrangement maintained as before in other respects, platina was attracted, and no revulsion exhibited. Palladium was very slightly repelled on completing the circuit, and then attracted; but on breaking the circuit was revulsively repelled, though so slightly that the motion was only just perceptible.

Zinc, gold, and lead were instantly repelled on completing circuit; continued to be repelled, though in a somewhat less degree, whilst the circuit was continued; and were decidedly attracted when the circuit was broken. Silver, copper, cadmium, and tin were repelled on completing circuit, but one side of the disc touched the polar piece, the other side being repelled; and on breaking circuit the side of the disc which had been repelled was strongly attracted. Bismuth and antimony were strongly repelled from a single pole, but no decided attraction was apparent on breaking circuit; the motion not being distinguishable from the effect of gravitation, which caused the disc to swing towards the polar piece, after the force which repelled it had ceased.

When the polar pieces are very close, that is, about 0.25 to 0.1 apart, and the disc of metal is so suspended that one half of it is without, and the other half within, or between the polar pieces, another series of phenomena present themselves. If the metal be amongst those classed as magnetic, or be magnetically affected by the power employed, it is, of course, attracted, and clings to one or other of the polar pieces, after the first motion, to be hereafter noticed, has subsided. If the metal be dia-magnetic it is repelled, and, in many instances, driven entirely out from between the polar pieces. At the instant of the completion of the voltaic circuit, the disc of metal moves transversely, as a pendulum, with a tendency to pass outwards from between the polar pieces; and on breaking circuit the disc moves transversely, but in the reverse direction, tending to pass within the polar pieces. Such motions are very remarkable, in that the direction of them is alike in all metals which do not become so strongly attracted by the magnetic influence as to cling firmly to one of the polar pieces. Thus they are exhibited in the same direction with

platinum and palladium as with antimony and bismuth. But they are exhibited with greater force by those metals which decidedly show revulsion. Silver and copper discs are so strongly drawn within the polar pieces on breaking circuit, as frequently to pass out at the other side of the polar pieces.

As it appeared to me that these transverse motions were, to a considerable extent, independent of the other phenomena of dia-magnetism, it occurred that they were produced by secondary electrical currents, induced in the disc of metal; and to ascertain whether this could be demonstrated experimentally, I took a piece of copper wire, about one-fiftieth of an inch in thickness, and folded it so as to make a flat spiral of the size of the discs previously employed, and of which the folds did not touch each other. On suspending the insulated spiral between the polar pieces it did not exhibit the transverse motions, nor the short and quick movement on completing circuit; or the revulsive motions on breaking circuit. But on constructing another spiral similar to that before-mentioned in all respects, excepting that part of the wire from the centre was prolonged, and put into connexion with the other end, forming the external fold, the motions, both revulsive and transverse, were exhibited in like manner as if a disc had been employed, though more feebly, as might be expected, inasmuch as any electrical current would have to pass through a considerable length of wire, and would meet with much greater resistance than it would have done in the disc.

Several discs of copper wire, folded in various manners, were tried with the like results; for if the folds were insulated, and the ends not in contact, so as to form a circuit, revulsive motions were not exhibited; but if the two ends were connected, then the revulsive motions, attractive or repulsive, according to the circumstances, or the lateral motions, were exhibited as before-mentioned.

Most of the before-mentioned experiments were varied by using an electropeter, which enabled me very rapidly to discontinue and renew, and also to change the direction of the current exciting the electro-magnet; but without obtaining any results worthy of particular description, in addition to those already stated; as reversing the voltaic current did not produce effects on the suspended disc essentially different from what would have been exhibited by the discontinuance and immediate renewal of the current in the same direction.

The arrangements of the battery were also varied by using six pairs of elements of zinc and platinised silver, each having about 1.5 inches acting surface, in addition to the four cells of nitric acid battery, so arranged that when the more powerful current of the latter was discontinued, a comparatively feeble current continued in some instances in the same, and at other times in the reverse direction. These arrangements did not, however, give any particular results, further than that the effects, on discontinuing the powerful current, were perhaps somewhat diminished; showing that the freedom of the electro-magnet from residual magnetism is only of slight importance in dia-magnetic experiments, and that the motions produced on the suspended disc are due to the exaltation or depression of the magnetic force, and not to its entire cessation.

I also repeated some of the experiments of Professor Oerstead, an account of which was recently published in the *Philosophical Magazine* for 1849, vol. xxxiv. p. 81, in which he pointed out that when the polar pieces are brought very closely together, and a small bar of dia-magnetic substance is suspended immediately over the space separating the polar pieces, the suspended bar may assume either the equatorial or the axial direction. I found this the case with bars of various metals; and that these would take

the apparently reverse position—that is, axial for equatorial, or the equatorial for axial—instead of that which they would have taken if suspended between the faces of the polar pieces. If I do not misunderstand Professor Oerstead's deductions from these phenomena, he seems to have supposed that under such circumstances the metal acquired a different state, or a different direction of polarity in regard to magnetism, when suspended above, than between the polar pieces. It did not appear to me that this was the case, but that all apparent anomalies might be most easily explained by considering the direction and distribution of the magnetic forces exhibited by the polar pieces when distant and when close. On examining the polar pieces when separated about half an inch, by means of a very small and delicately-suspended disc of metal, either magnetic or dia-magnetic, it is ascertained that the magnetic force is most strongly developed, as though upon the exterior of such parts of the polar pieces as are opposed to each other; and that with polar pieces having plane surfaces, similar to those employed in these experiments, the magnetic force is more strongly developed at the corners of the opposed parallelograms. Thus it appears that when the polar pieces are distant, the line about which the disposition of the magnetic force is a maximum, is the axial line joining the two poles; but that when the polar pieces are brought very closely together, the greatest intensity of the magnetic forces is upon the two parallel lines on the upper edges of the opposed faces; and the two parallel lines being very near each other, may be considered as a single line in regard to their action upon the suspended piece of metal; and it may also be observed that the forces are greater towards the extremities than at the middle of such lines, so that the direction of the magnetic force becomes virtually equatorial instead of axial, in regard to

a bar of metal suspended immediately above the space separating the polar pieces.

In experimenting with small bars of lead, the length of which was three-quarters of an inch, breadth one-tenth, and thickness one-twentieth, and suspended above instead of between the polar pieces, I found that the like changes from the magnetic to the dia-magnetic states, and also the like revulsive motions, were exhibited, as when the metals were suspended between the polar pieces, due consideration being given to the change in the direction of the distribution of the magnetic forces, when the polar pieces were made to approach very closely to each other.

Whenever a series of facts are discovered which are not readily explicable in accordance with generally received theories, we may hope to acquire some useful correction of such theories, by either extending their applications to the newly discovered facts, or by altogether remodelling such parts of the general theory as may be inconsistent or inapplicable. In this point of view I conceive that all facts relating to dia-magnetism are particularly interesting, and may reasonably be expected to lead to an increased knowledge of the physical properties of matter. I hope this part of the subject will be pursued by those most able to do so; but as I have, in the course of my experiments, been unable to explain the phenomena according to the theory of Ampere, and have, therefore, attempted to discover the laws on which the observed phenomena were dependant, I will endeavour to communicate the ideas which then occurred to me. I feel, however, the more diffidence on this subject, as my opinions are not quite in accordance with those generally received in regard to electro-magnetism.

According to the theory of Ampere, the magnetic condition is induced by the action of electric currents, circulating around the particles of matter; and it has been shown

that both permanent and electro-magnets, under certain conditions, induce electrical currents. The circumstances under which a magnet or an electric current induces another current, have been very fully investigated, but we do not find any instance in which the direction of the induced current is affected by the nature of the matter in which the induction takes place. If it were only necessary to account for those magnetic phenomena in which the matter subjected to electrical or magnetic force becomes affected by such polarity that it is attracted by the pole of the magnet to which it is opposed, we might have no great difficulty in the general application of Ampere's theory; for we may easily conceive that currents circulating around the poles of an electro-magnet induce other currents in a neighbouring mass of iron, under the influence of which the latter is affected by such induced electrical currents as to be attracted by the electro-magnet; but I am not aware of any extension of Ampere's theory by which it can be explained how it happens that when, in lieu of the piece of iron a piece of bismuth is substituted, the latter is repelled, instead of being attracted.

The theory of Ampere may be applied to explain the magnetic or dia-magnetic state of some bodies, for instance, platinum and bismuth, by supposing that in the platinum a current is induced in one direction, and in the bismuth in the opposite; but with regard to the phenomena exhibited by lead, the difficulty is much greater. We may suppose that the magnetic condition of lead is caused by a current in one direction, and (passing over the difficulty of accounting why a feeble magnetic force should induce a current in one direction, and a greater degree of magnetic force a current in another direction) that the dia-magnetic condition is caused by a current in the contrary direction to the assumed current in the magnet. But we have the

intermediate state, not neutral, but sluggish (for a more elegant term has not yet been suggested.) I cannot conceive any manner in which we may account for such assumption of the sluggish state according to Ampere's theory, unless we suppose two currents in opposite directions in the same piece of metal, and at the same time; a supposition which is contrary to all observed phenomena or laws of electric currents. We should, nevertheless, have some difficulty in explaining the effects produced, for under whatever circumstances we have hitherto observed the production of such currents, we find that they are transient, and only continue during such time as the inducing force is either increasing or diminishing. We find, on the contrary, that the magnetic force does not in general rise to its maximum until after the lapse of a longer period of time than an electric current, induced under similar circumstances, would have continued. This consideration becomes still more forcible in regarding the phenomena exhibited by zinc, under the influence of forces which are little more than sufficient to render it dia-magnetic. The zinc is at first attracted as magnetic, but slowly becomes dia-magnetic, appearing to pass through an intermediate state, in which it appears very little sensible to either attraction or repulsion. We have also to consider the phenomena of revulsion, which might be explained by the hypothesis of induced currents, if the revulsion always took place in the like manner and direction. The revulsion is sometimes attractive, and sometimes repulsive; if, therefore, these phenomena be due to induced electrical currents, such currents must be in different directions. I have, however, pointed out that the phenomena of revulsion are intimately connected with electrical currents, as the revulsive effects are not exhibited with the insulated disc of wire, in which the completion of a circuit is prevented. On these, and similar considera-

tions, I have doubted whether in any case an electric current directly induces a current in a parallel conductor, and formed an hypothesis, by which I conceive many of the phenomena, otherwise discordant, may be accounted for.

The full development of my hypothesis might lead to the introduction of some ideas, in relation to the constitution of matter, foreign to the subject of this communication: I must, therefore, assume certain positions which admit of considerable doubt.

I will assume that matter is composed of ponderable atoms not in absolute contact, but separated by intervening space, or by imponderable matter; and I consider it unnecessary at present to determine, or even to consider, whether the space intervening between the ponderable particles be occupied by an imponderable element, as caloric, by an ether, as assumed in some of the theories of light, by vibration, as assumed in other theories, or by any other manner or form of mere force. Such compound constitution of matter is necessary, in order to admit of any explanation in what manner matter may become affected by any kind of polarity.

I will also assume that when matter becomes polar its molecular condition is disturbed; that when the molecular constitution of a body is disturbed electricity is developed, though the exhibition of any electrical phenomena will depend on the circumstances permitting the establishment of an electric circuit, as certain conditions of matter will, in many cases, so permit the return of the electricity through a short circuit within the matter in which it is developed, that no external effects can be observed.

I also assume, as a correlative circumstance, that an electric current produces molecular disturbances, not only in the matter conducting the current, but also in the contiguous matter, whether conductor or non-conductor; that

the disturbance of the particles not engaged in the actual conduction of the current is a kind of polarity continuing during the uniform transmission of the current through the adjacent conductor. I consider that such polarity either is or is not accompanied by a molecular disturbance of the particles of the body; and also that when such polarity ceases, another molecular disturbance again takes place, the particles returning to their normal condition. Such molecular disturbances, according to my hypothesis, occasion electrical currents, if they occur in bodies favouring the circulation of such currents, that is, in conductors. Thus I conceive that I can explain how a current, at the moment it is established, or during such period as its force is increasing, induces a current in an adjacent conductor; not by direct induction, for direct induction should produce a continuing current, but that the particles being polarised, or being subjected to a molecular disturbance, induce a secondary current during such period as the disturbance is being effected; and that the molecular disturbance having attained a maximum, the secondary current ceases; that when the primary current ceases or declines, the polarised atoms regain their former condition, such change being also in the nature of a molecular disturbance, which induces a secondary current in the opposite direction to that first induced.

To apply such hypothesis to the explanation of magnetic phenomena, it may be assumed that such polarity is produced; that in each particle the ponderable atoms are unequally distributed with regard to the imponderable, (still disregarding the consideration whether such imponderable be material, or be vibratory, or a mere condition of force), and that such polarity is the cause of attraction or repulsion according to the direction in which such polarised atoms, in two bodies, are presented to each other.

In electro-magnets, I conceive that such molecular dis-

turbance or polarity is in some manner disturbed by the influence of the neighbouring electric currents; but our knowledge of the molecular constitution of matter is as yet much too limited to admit even a supposition being formed as to the precise manner. In permanent magnets of hard steel, I think that a similar polarity is effected by the process, whatever it may be, by which the magnetism is induced; that such polarity is retained for an indefinite period, as the hardness of the material prevents the polarised particles returning to their normal condition.

We may conceive that a body (A) affected by magnetic polarity, may affect an adjacent body (B), producing a similar polarity, by arranging the particles of B in like manner as those of A as regards polar direction; that of the particles of A, the ponderable matter is distributed in one direction, the imponderable in another; but on substituting another body (C) for B, the arrangement of particles may be so different, in that the imponderable is more attracted, that the imponderable B will exhibit phenomena as a magnetic bar dia-magnetic; and we may conceive, in accordance with Professor Plucker's law, that the effect of the dia-magnetic force on the attraction causing it, affects the ponderable and imponderable atoms of matter in a different ratio; that a certain development of magnetic force attracts the ponderable elements more than the imponderable, producing magnetic polarity; that a higher development of magnetic force attracts the imponderable more than the ponderable, producing dia-magnetic polarity; that in an intermediate state both the ponderable and imponderable atoms are attracted so equally, that neither magnetic nor dia-magnetic polarity is produced; but, nevertheless, such molecular disturbance is caused, that powerful revulsive currents are developed on the discontinuance of such affection of the atoms.

Having considered how the magnetic and dia-magnetic condition of matter may be accounted for, the revulsive motions require considerable attention. These cannot, in any manner which I can conceive, be explained on any hypothesis, either of molecular action, independently of electric currents, or of electric currents directly induced by the magnetic force. If the revulsive motions were supposed to arise on the ceasing of the molecular disturbance, under such conditions that on the ceasing of the constrained molecular condition, the atoms, by a species of libration, spring into the contrary arrangement, passing beyond the normal condition, and thus that magnetic action is succeeded by temporary dia-magnetic repulsion, and dia-magnetic action is succeeded by temporary magnetic attraction, we have, in addition to many other objections, the important one that the force exhibited by the revulsion is not in proportion to the force supposed to originate it; for, as appears from the before-described experiments, when the magnetic force is considerable, as in palladium, the revulsive force is very slight; and when the magnetic force is slight, as in zinc, the revulsive force is comparatively very considerable. I have also shown by the experiments with spirals of copper wire, that the phenomena of revulsion and of the transverse motions are closely connected with the conduction of electric currents. We do not perceive these motions when a complete circuit is not afforded for the electric current, nor have I been able to observe them in bodies of inferior conducting properties to the metals.

The conducting power of the metal may, to some extent, affect the amount of revulsive force exhibited; but the property of revulsion clearly does not depend on this, for platina is a much better conductor of electricity than lead, and in platina we have little, if any, revulsion, whilst in

lead it is strongly exhibited; and in copper, which is considered to be the best known conductor, we have the revulsion very strongly exhibited. We cannot account for the revulsive motions on the generally received hypothesis of induced secondary currents, for, as I have before pointed out with regard to magnetism and dia-magnetism, such currents would only account for revulsion in one direction.

I conceive that the revulsive motions are caused by the magnetic affection of the piece of metal induced by electric currents, which currents are produced in the metal on the subsidence of the molecular disturbance, previously caused therein. It would be difficult to say how, on the determination of the attractive polar condition of a piece of metal affected magnetically, a current should be produced which should reverse the polarity, and develope a greater force in the opposite direction; but it is not the strictly magnetic polarity which precedes the repulsive revulsion, but that molecular condition which takes place in the intermediate or sluggish state, that, as I conceive, is the immediate cause of the induction of the current. Thus with platina and palladium we have little, if any, revulsion exhibited, these being affected only magnetically, and not passing into the intermediate state; whilst with all the metals which are magnetic under one amount of force, and become dia-magnetic under an increase of force, and all of which pass through the intermediate or sluggish state, we have the revulsive motion strongly developed. It must also be observed that in the metals, antimony and bismuth, which are not affected as magnetic previously to becoming dia-magnetic, the phenomena of revulsion are not exhibited.

I therefore submit that the metals which have been observed to change from the magnetic to the dia-magnetic states, are subject to three different conditions of molecular arrangement; first, one of magnetic polarity, and which

on its cessation only induces a feeble electric current; the second, the intermediate or sluggish state, in which the metal is not polarised so as to be either attracted or repelled by the magnet, but in which there is, nevertheless, so great a molecular disturbance that very powerful electric currents are induced on its discontinuance; and the third, in which the particles are so polarised as to be repelled by the poles of the magnet which has induced such condition, but which last condition does not, on its discontinuance, induce powerful electric currents.

I have not formed a decided opinion whether the attractive revulsion exhibited by a changeable metal, when the force which has induced in it the dia-magnetic condition has ceased, is due to the action of an electric current in the contrary direction to that which occurs on the change from the sluggish state; or whether the particles of metal, in assuming their normal condition pass through the magnetic state, and are then attracted.

I leave the consideration, by what scheme or arrangement of particles such three conditions of matter and their polarities may be accounted for, as subjects for future investigation.

PROCEEDINGS

OF THE

GEOLOGICAL & POLYTECHNIC SOCIETY

Of the West-Riding of Yorkshire,

AT THE FORTY-FIRST MEETING, HELD IN THE COURT-HOUSE, BARNSLEY, ON TUESDAY, OCTOBER 29TH, 1850.

The Right Hon. EARL FITZWILLIAM, F.R.S., President, in the Chair. The annexed statement of the accounts for the past year was submitted by the Secretary:—

STATEMENT OF THE RECEIPTS AND EXPENDITURE,

From 1st January, 1850, to 29th October, 1850.

DR.	£.	s.	d.	CR.	£.	s.	d.
To Balance in Treasurer's hands..	49	12	7½	By Printing Reports, &c.	18	3	0
To SUBSCRIPTIONS from—				„ Advertising Meetings.....	3	9	10
1 Member for 1843 at 13s.	0	13	0	„ Lithographing Plates.....	4	18	0
1 „ 1844 „	0	13	0	„ Expenses of Meetings	1	13	0
1 „ 1845 „	0	13	0	„ Collecting Subscriptions	1	2	3
1 „ 1846 „	0	13	0	„ Postage Stamps and Postages..	1	19	4
2 Members for 1847 „	1	6	0	„ Books and Stationery.....	1	7	0
4 „ 1848 „	2	12	0	„ Rent of Museum Room, &c....	20	0	0
19 „ 1849 „	12	7	0	„ Salary of Assistant Secretary ..	50	0	0
65 „ 1850 „	42	5	0	„ Sundry Small Expenses	0	18	1
1 „ 1850 at 10s. 6d...	0	10	6	„ Balance in Treasurer's hands ..	9	14	7½
Donation from J. W. Child, Esq.	2	0	0				
	£113	5	1½		£113	5	1½
Assets about	£98	17	6	Liabilities about.....	£20	19	6

After a few preliminary observations, his Lordship called upon Mr. West to read the first Paper, entitled:—

COMPARATIVE VIEW OF CIRCUMSTANCES ATTENDING SOME OF THE EXPLOSIONS IN COAL MINES. BY WILLIAM WEST, ESQ., F.R.S., ASSOC. CIV. ENG.

I am not about to deliver, on this occasion, a lecture on the ventilation of coal mines; although I think that it would be highly useful to viewers and other superintendents, if, without expecting to arrive thereby at any nostrum or secret, they listened occasionally to a plain statement of the most impor-

tant facts respecting fire-damp, choke-damp, atmospheric air, combustion, rarefaction, and other agents in those frightful accidents against which they have to guard. Still less have I either nostrum or secret to reveal.

But in the course of preparing two or three such lectures, not yet delivered, some circumstances presented themselves, which, however, singly, well known or obvious, have scarcely met, in connection with each other, with all the attention which they deserve. The report of Sir Henry De la Beche and Dr. Playfair,* it is true, is so able and so comprehensive, that it supersedes much which I might otherwise remark. But several minute and accurate accounts of explosions of later dates have appeared since their general report or essay was written. These enable me to draw inferences from a greater number and variety of well-recorded instances than they had then before them. Many circumstances are only incidentally mentioned, or to be gathered from casual remarks, and are therefore by no means plain on once reading. The result on my mind is a persuasion that tendencies to a dangerous condition exist in mines reputed to be comparatively safe, and that these tendencies are so numerous and vary so suddenly, that no degree of precaution of one kind can exclude the necessity of attention to others.†

The appointment of inspectors of coal mines, and the subjecting them to regular inspection, will, we may reasonably hope, diminish the frequency and the extent of these disasters. I do not anticipate that they will ever entirely cease. But this step, the appointment of inspectors, ought rather to increase than lessen the interest which scientific bodies like this should take in the discussion of past, with a view to the prevention of future, misfortunes. The same records which

* Collieries' Report, 1847, p. 4.

† I use the term "mine," though "pit" is more common, because the latter would cause some ambiguity, since "pit" is in several of these accounts used also for "shaft." It is so in "Collieries' Report," pp. 47, 48, 49.

I have so repeatedly and minutely studied must furnish materials for study to those inspectors.

Again, it is on every account most desirable that the Inspectors should operate on proprietors rather through persuasion and representations than through the gross machinery of compulsion, fines, and prohibitions. On the Continent it is so.* In this country Government or official interference is viewed with more jealousy. We are less accustomed to it. Recommendations from authority are null unless supported by public opinion, and public opinion does not express itself, is not even formed, unless a strong interest is felt by a large portion of the community. All who consume coal, indeed, ought to have a deep, though it must be a melancholy, interest in these occurrences. Many a time when

> "Shadows from the fitful firelight
> Dance upon the darken'd wall;"

many a time when

> "We stir the fire, and close the shutters fast,
> And calmly welcome sober evening in;"

may we revert to the thought that, in obtaining the means of domestic cheerfulness for us, numbers from time to time have met with death in horrid forms, and widowhood and misery have been suddenly spread among other hearths. When such recollections obtrude, no feeling heart, no thoughtful mind, but must bid good speed to every inquiry calculated to diminish this source of human woe.

One of the first circumstances evident on comparing these accounts is, that few as they are, they embrace coal fields widely scattered, and very different in many respects. Seven explosions reported on comprehend Durham, Yorkshire,

* In the "Darley Report," at p. 6, Mr. Tremenheere says, "In France, Belgium, and Germany, very nearly all the alterations thought right by the Inspectors, in order to the safety of the people, are obtained by frank and friendly representations alone. To use their own expressions, 'they act by persuasion and the force of conviction far more than by force of law.'" Similar statements are to be found in many other passages of his reports.

Lancashire, Warwickshire, and Monmouthshire, five districts as remote from each other as could be wished if we had made a selection. (See table.) The thickness of the beds varies from four or five to thirty feet. (See table.) The coal at Oldbury is much shattered.* At Risca† and at Coppull‡ it is very hard, so that it is considered necessary to blast it with gunpowder. At Haswell, also, though the report does not mention it, gunpowder was employed; also at Jarrow.§

Let us turn to some general remarks. Pits long and extensively worked are justly considered more dangerous than such as have been recently opened. Yet a new pit is not necessarily safe. Deep pits are generally considered more unsafe than those nearer the surface.‖ There is little reason why it should be so, unless where depth implies old or extensive working. But mere depth of shaft is favourable to ventilation where cupolas or furnaces are employed, as supplying a long column of rarefied air. The depths reported vary from 300 yards at Haswell and at Jarrow, to 130 yards and 75 yards at Darley. Some seams are much more fiery, that is, give out much more carburetted hydrogen than others. These undoubtedly are the most dangerous, and require especial care. But I find the character of the seam at Haswell to be "less fiery than many."¶ At Darley "but little firedamp."** "No great quantity of Gas."†† So that a mine bearing a tolerable character may be the seat of repeated explosions.

That habit, and comparison with yet greater risks, can lead men to apply very gentle terms to the degree of danger in their own case, is shown in the instance of Jarrow, respecting which it is said,—"Not reputed to be very fiery, but the quantity of gas is very considerable."‡‡ In this "not

* Report, 1847, p. 44. † *Idem*, p. 23. ‡ P. 48. § P. 22.
‖ Edinburgh Review, No. clxxx, April, 1849.
¶ Philosophical Magazine, vol. xxvi., p. 19. ** Report on Darley, p. 10.
†† Ditto, p. 1. ‡‡ Report, p. 15.

very fiery" mine six explosions had taken place in twenty-eight years, causing the death of 140 persons.*

Some may be inclined to sum up and dismiss the subject with—"You must have good ventilation." But the reports, in this respect, as to some of those places where the worst explosions of late years have occurred, show the compatibility of ventilation deemed generally "good" with fatal deficiency in the hour of need. At Haswell the ventilation is reported as "most admirable."† The ventilation at Jarrow seems to have been good in the estimation both of the scientific visiters and of the workmen. Dr. Playfair says, "The testimony of those at work in the pit four hours previous to the explosion shows it to have been free from fire-damp, and well ventilated."‡ John Atkinson, a hewer, who lost a son, torn limb from limb, says, "The air was very good."§ Jacob Barnborough lost a son by the explosion, yet he says, "The air in the pit was very good; I had a son lost, but I acquit the masters of all blame in his death; there was nothing wrong in the ventilation."‖ Emanuel Dufty, "The pit was quite chokeful of air, and in beautiful working order."¶ Another, "The air was quite good, and plenty of it."** Yet, a few hours later, forty persons were deprived of life. At Risca, the ventilation was avowedly in a transition state, but no pains nor expense were spared; and a workman says, "There is famous air in the pit."†† At Coppull, Mr. W. W. Smyth, the Government Mining Geologist, says, "I found the ventilation in very good order, and have no reason to think that it had been neglected before the accident occurred."‡‡ Ventilation, therefore, such as is deemed "good" and "sufficient," does not by itself produce security.

* Report on Darley, p. 17, Note by Dr. Playfair.

† Philosophical Magazine, vol. xxvi., p. 22. ‡ Report, P. 16. § P. 17.
‖ P. 18. ¶ P. 19. ** P. 18. †† Abiram Gething, p. 26. ‡‡ P. 49.

But a further consideration of importance presents itself. So far as the quantity of air conveyed to a certain extent of workings is concerned, (and the abundance of this, probably, would be the common idea of good ventilation), a limit, in most of these mines, seems to have already been reached. This limit arises from the inconvenience of too great velocity of current near the entrance, before the splittings take place, and in the narrower passages afterwards. At Jarrow, a witness says, "The candles swealed away." "I burnt six candles in my shift. There is more air goes through the Low Main (the seat of the explosion) than through the Bensham (the seam next above), and that has to do with it."* At Risca, "There were two or three, perhaps, working with lamps, because they could not keep their candles lighted for the draught."† James Padfield says, "You can hardly carry a candle in the wind-ways."‡ Richard Cottle says, "I was obliged to use a lamp to keep a light, the wind was so strong."§ At the Oaks, John Pickford says, "I have been obliged to take my cricket to keep the wind off my candle."‖ And Richard Hodgson, "It would sweal away a candle in two or three minutes any day."¶ Even at Darley, where the practical and scientific gentlemen agreed in considering the quantity of air to be but about a third part of what was necessary,** two witnesses state that the men complained of the wind swealing their candles,†† and their burning too many in consequence. Where this limit is thus already reached, we must look for additional security from some other source than greater strength of current. To attempt to compel the formation of new or additional shafts would be an interference of

* Report, P. 18, Jacob Barnborough. † P. 36, Mr. Joseph Green.
‡ P. 38. § P. 39. ‖ P. 64. ¶ P. 65.
** Mr. Tremenheere, Darley Report, p. 1, and Mr. Smyth, p. 11.
†† Mr. Locke, the colliery agent, and George Addy, fireman. *Supplement to the Leeds Mercury*, 3rd February, 1849.

the kind which provokes resistance, since it might involve enormous additional expense. Yet it remains for the calculation of proprietors themselves whether explosions and their consequences do not exhibit a large per contra figure. Jarrow Mine, six times wholly or partially stopped, six times to be repaired and restored, in twenty-eight years, must have furnished some set off to the expense of providing different shafts for different purposes, instead of one shaft divided into three parts by wooden brattices. To place the shafts, at the first laying out of the workings, at proper distances instead of close together, would not involve, in many instances, much additional expense; and it is of great importance. At the Oaks, where the upcast and downcast were only nine feet asunder, the men and boys who reached the bottom of the shaft were kept between the danger of suffocation by after-damp, or being crushed by stones, with very few yards of fresh air. At Darley, where the arrangement of the ventilation ensured a supply of air for a considerable distance, many of those carried out as dead subsequently recovered.* At Jarrow, Jacob Barnborough, hewer, says, "If they (the masters) gave us two shafts instead of one, it would give us a better chance of escape after an accident.†" Every arrangement which tends to keep up the current of fresh air after an explosion, or to facilitate its speedy restoration, is of great importance in lessening the extent of injury to life. To nothing do the accounts more generally testify than that the greater part of the deaths take place from the choke damp, or after damp—carbonic acid gas. I find from my examination of the evidence, that at Risca it seems doubtful whether more than four or five lives were destroyed by burning, out of thirty-five lost. We have instances of stupor being produced, and recovery following after the

* Report on Darley, p. 10. † Report, 1847, p. 18.

lapse of many hours. At Jarrow, William Cranstoun recovered, after being insensible, as he believes, for thirty-six hours.* And some recovered after shorter periods in other cases. While I was writing this paper another lamentable explosion took place at Oldham (October, 1850). As the newspaper account gives many of the particulars which I have endeavoured to ascertain in the other instances, I have added it to my tables, although not included in the official reports.

It strikingly proves the mischief of one shaft, divided by bratticing, when an explosion does happen. The force of the blast extending to the shaft, the bratticing was in part blown out, and the entire stoppage of ventilation in the passages of the mine was but a part of the evil consequent on this. The wreck of the brattice formed a pile at the bottom of the shaft, six or seven yards high, and sufficiently solid to prevent the cage from descending for that distance. Think of men and boys, burnt, enfeebled by the effects of choke damp, and distracted by terror, climbing up a loose pile of broken wood, for six yards! Several were, in fact, maimed or hurt by falls. It was four hours before three cages could be got to the bank! One boy, clinging, in his eagerness, to the first cage after it was full, lost his hold from exhaustion, and was killed by the fall. Lamps alone were used. The immediate cause of the explosion is believed to have been a fall of roof upon a lamp which the hurriers had left upon the ground in one of the bays, while they went to the shaft bottom with a journey of coals. The seam then in working had only been opened two years, and the workings were not extensive, only 55 yards to the north, and 140 to the south from the shaft.†

I now approach what has been elsewhere a topic for many controversies—the wire gauze lamp. If it become so here,

* Report, 1847, p. 22. † *Leeds Mercury*, October 12th, 1850.

it will be, I believe, through others than me. For I am willing to yield for the time every point except one, that flame surrounded by wire gauze is less dangerous than a naked light. Whether Sir Humphry Davy was really the first inventor of the instrument named after him, whether previous attempts or later improvements are really superior to "The Davy," may, so far as my object is concerned, be left unanswered. The case is narrowed with me to "Wire Gauze *versus* Candle." Nor does it need, for a decision in favour of the Davy, that it should be safe under all conceivable, or all actually occurring circumstances. I think it a pity that the name of "Safety Lamp" was ever given to it; as "The Davy," "The Stephenson," "The Clanny," "The Upton," would not, by their mere names, have raised ill-founded expectations. There is certainly one way in which a lamp, safer than candles, but not absolutely and infallibly safe, may do mischief. This is if, in reliance upon its efficacy, work be carried on by its means in more dangerous situations, or with fewer provisions for safety, or less caution in other respects, than would have been the case were it undiscovered or discarded.* But we cannot ascribe to this any one of the particular explosions now under consideration. At Haswell, lamps alone were employed; but since the seam is stated to be

* "The Committee of 1835 pointed out that more persons had lost their lives by colliery explosions for the eighteen years succeeding the introduction of the Davy safety lamps in 1816, than in the eighteen years preceding the invention, and accounted for this fact by the working of numerous 'fiery' seams of coal, which had, in consequence of the assumed security of that lamp, been undertaken, and the abandonment of many precautions considered requisite when candles were commonly employed in collieries." (Report, 1847, p. 8.)

"An unfounded confidence in the lamp has been productive of great loss of life. First, by causing work to be carried on in dangerous mines, without other measures of security against explosions; and still more unfortunately, by leading to a serious limitation in the number of shafts." (*Edinburgh Review*, No. 180, p. 545.)

"less fiery than many," and its ventilation "most admirable," it is evident that it would have been worked, and worked in the same general manner, had the safety lamp been unknown. At Risca, where the general use of gunpowder would have seemed to render the precaution of lamps almost vain, only about one-third of the lights were lamps. We cannot, therefore, suppose the use of these to have caused any neglect in other respects. And at the Oaks, where candles were "chiefly" used, and at Jarrow Low Seam, Oldbury, Coppull, and Darley, where candles alone were employed by the hewers, and lamps confined to the doggy or fireman, for purposes of inspection merely, no dependence on the safety lamp can have influenced either the opening of the mines, their modes of working or of ventilation, or the conduct of the workmen. If "many precautions formerly considered requisite" have really been abandoned since the discovery of the lamp, it confirms what I am endeavouring to impress upon you, that mischief will attend every undue dependence on any one means of safety. The real question is, then, "Is wire gauze *any* protection?" "Is the lamp safer than a candle?" On these heads I have no doubt. The nature of the case, abundant observation, the testimony of scientific men, even that of pitmen, (who, however, prefer the greater light of candles,) all concur to show that, to a certain extent, wire gauze is a protection, and the lamp safer than a candle. Every time that the cylinder is filled with flame, that flame would extend, and an explosion follow, but for the wires. Sir Henry De la Beche and Dr. Playfair three times expressed their opinion that "much additional security is obtained by the proper use of the original Davy, or its improvements.*" That they "cause security in all cases where proper care is employed, though they may not be absolutely safe under

* Report, 1847, p. 8.

all circumstances."* That "safety lamps, properly used, do effect much security in the working of coal."† Dr. Playfair again expresses his "conviction that the accident at Jarrow would, in all probability, not have occurred, had lamps been employed instead of candles" in the Low Main.‡

I leave to colliers the question between higher wages, with greater danger, and lower with safety; to coal masters, whether to allow all or any part of the increased wages or allowances claimed for the use of lamps. But the testimony in favour of lamps is not confined to theorists or men of science. A workman at Jarrow, after making the usual complaint of the Davy being "so dark," is asked, "You have had long experience in pits; did you ever know an accident take place where the Davy was fairly used?" He answers, "Never."§

We have further some very interesting facts, furnished not by a philosopher writing in his study, but by a viewer at Walker Colliery, near Newcastle-on-Tyne. On November 13th, 1846, when approaching a slip dyke, a mass of coal was displaced, weighing about eleven tons. On the displacement of this block, and the discharge of fire-damp that followed, being observed by the two men working in the drift, they immediately extinguished one lamp, (the other having been put out by the accident,) informed the other men in the pit, and retired to the shaft. The air rendered foul in an instant was ascertained by subsequent measurement of the passages to which it extended to be more than 40,000 cubic feet. A second violent discharge took place from near the same spot on the 10th December. Judicious precautions had been taken to prevent mischief. While the men were breaking down a portion of roof, to enable the tram-road to be carried up, the "danty" or broken coal in the slip of the dyke, above where a bore-hole had just been made, was

* Report, 1847, p. 9. † P. 11. ‡ P. 17. § P. 19, Emanuel Dufty.

violently forced out, with a noise, continuing for some time, which resembled the blowing off of an immense high-pressure steam-engine, and a violent discharge of gas, that extended over a distance of 640 yards in length, with an area of 86,000 feet. At 400 yards from the point of issue it was met by one of the deputies. Though he drew down the wick of his lamp, the gas continued to burn for ten minutes, heating the wire red hot, and leaving the particles of coal attached to the wire gauze burnt quite red. At the end of the 640 yards, the gas burnt within the lamps of five men and boys there employed, until they extinguished them in a sump or pool at hand. There can be no doubt, therefore, that the explosive mixture extended still further. Where would the workmen on these occasions have been but for the lamp?

A low state of the barometer is obviously favourable to the discharge of gas. Many practical men are aware of this. The most curious instance of its operation is that mentioned as occurring in a pit of Mr. Jobling's, of Jarrow, where, with a low barometer, inflammable gas was habitually noticed to issue from the coal; while, when the barometer was high, common air was observed to enter the coal at the very same fissures. But how many of those whose employment it is to inform the workmen when caution is requisite possess or understand a barometer?

I was the more encouraged to bring forward this paper by finding, as I proceeded, that several circumstances then acquired an interest, in connection with others occurring elsewhere, which they by no means possessed while detached. But a cursory perusal of the accounts, by persons ever so well acquainted with the subject, would not exhibit these in their full force and bearing.

I have attached to this paper some tables,* which include

* The Diagrams on the wall contained these Tables, a little abbreviated.

most of the particulars admitting of comparison. On some desirable heads the accounts give little information, such as the quantity of air computed to enter, the kind of roof and of floor, &c. And I should wish to see inserted in future accounts several heads respecting which our present accounts furnish no materials. Such are—the speed of the air current in the principal air passages; the amount, and kind, and period of sinking in the country above, &c.

It is remarkable that at three of those places from which we have the most particular accounts, coal getting had been suspended shortly before the accidents. At Jarrow, for six days, until four days before the explosion. At Risca the day before,* and at Darley the day before,† respectively. The events might create a doubt whether the ventilation had in each case been carried on as actively during the intervals as if the usual labours of the pit had been continued. This subject was not remarked on by the Government Commissioners, or, so far as appears, by the Coroner, in either case. Whether, in these instances, it be a significant fact, or only an accidental coincidence, it is quite clear that ventilation should never slacken. The gas is constantly issuing in pits subject to it, and it will accumulate during the absence of the men, to be ready for any contingency favourable to its inflammation after their return, unless it be in the mean time, and as fast as it issues, diluted and carried off. It is also observable that at Risca both the firemen had, through illness, or being engaged elsewhere, entrusted, on that day, their usual duties to others. At Darley also there had been a change, though both must be supposed equally skilled. George Addy, fireman, who had gone down to "try the fire" daily for two years, being ill, Joseph Thompson, the bottom steward, took his place. I attach no importance, however, to this. Let me remark that, in commenting on

* Collieries' Report, p. 36. † Darley Report, p. 10, and *Leeds Mercury*.

the circumstances attendant on any of these sad catastrophes, I mean no unkind or unhandsome reflection on any of the parties concerned. Those circumstances are as much matter of history as the explosions themselves, and as such are open to fair and candid remark. The "scaling off" of a portion of fresh air to feed the furnace, recommended by Mr. Warington Smyth,* and conducting the return air into the upcast shaft at a safe height above the fire, occurs as being practised in none of these reports. For want of this the fire in the furnace at Risca, and that at Darley, had to be put out just when a powerful current of fresh air was most wanted. And time, any moment of which might make the difference of life or death to some perishing sufferer, was spent in giving the needful directions, or waiting for their fulfilment. Double doors, allowing a corve or tub to enter the space between, and the first to be shut before the second is opened, prevent even the momentary interruption of the proper ventilation, and lessen the danger of a disarrangement of longer continuance from accident to, or neglect of, a single door. They are referred to by Sir H. De la Beche, but in the other cases the doors seem to be single. I believe they are very commonly so.

Oldbury appears to be the only mine where the upcast shaft was unprovided with a furnace. The catastrophe at this place produced a pamphlet, or rather a book, from a gentleman of that neighbourhood, which consisted almost entirely of the advice to build a tower over the mouth of the upcast shaft, thus increasing its length, and not to use furnace or fan, or any other artificial means of producing a current. I do not agree with him. Mr. Smyth recommends a tower or stack at Darley, where the up shaft is much shorter than the down, but would retain the furnace.

* Darley Report, p. 16.

I am not prepared to say that the employment of gunpowder should be forbidden, or that a seam which cannot be profitably worked without it should be abandoned. But I do think that the difference in cost between gunpowder and the pick alone should be considerable, to render blasting, on the whole, advantageous to the proprietor. That it forms of itself a naked light, and thus renders almost nugatory the use of lamps, is obvious. The sudden dislodgement of large masses of coal must favour sudden and rapid discharges of gas; while the loose, open, shattered condition, which it is the very object of blasting to produce, is no less favourable to keeping up the emission. When the coal is cracked and crushed, the pillars or "posts" left must either be larger, or they will be weaker, and not the coal alone, but the roof, will suffer, and those "falls" of stone or shale take place which are the causes of fatal accidents in various ways, and explosions among the number. It is not, then, alone a question between so many pounds of gunpowder on one hand and so many weeks' additional wages on the other; the coal shaken by blasting is also less valuable, from the readiness with which it falls into "slack."

Many considerations point to the goaf or wastes, or worked out portions of a mine, as the source and seat of explosions. The evidence, it is true, must be, from the nature and results of the calamity, inferential, rather than direct. What no living eye has witnessed, no living tongue can remain to tell. But in the Haswell Colliery, and in some other cases, satisfactory evidence as to where the gaseous mixture fired was afforded by the direction in which its force acted, and by the condition and situation in which the bodies of some of the pitmen were found; and the goaf of thirteen acres was there indicated as the largest reservoir of gas. At Jarrow the waste was clearly pointed out. At Risca the evidence

shows that the immediate cause of the accident was a man (William Curtis) going into the gob, where he ought not, with a candle.* At the Oaks, the upper class of witnesses, skilful in coal working, after hearing the evidence as to the finding of the body of William Walton, came to a like conclusion as to the cause of explosion there.† (See table.) But even before this they imputed it to a fall of roof in the south waste.‡ In either case the waste is concerned. From Oldbury and from Coppull I have not the reports of the evidence at the inquests; my information and my tables are therefore less full and precise as to those two than the others. At Oldbury, however, the "old sides" were professedly walled off, but two of the walls were "uncapped;" "probably not produced by the accident," says Mr. Warington Smyth.§ The old sides, or waste, therefore, most likely, were concerned. At Coppull, "the roof is free from gas, and even after it has fallen down, and left large cavities above, is said not to harbour any other noxious gas than 'black damp.'"‖ But if not given out by these cavities, it must collect in them. At Darley Mr. Tremenheere says, "The colliery was worked on a plan by which, as the coal was got out, large cavities were left, only partially filled up by the breaking down of the roof; and those cavities became so many magazines of explosive gas." In at least four mines, then, the great reservoir of inflammable gas was the goaf, and in three others very probably so. At Oldham alone was the gas collected evidently in the working parts, or bays, of the mine.

We need not wonder, then, seeing the special danger which attends the goaf, that Faraday and Lyall, at Haswell, directed their attention almost exclusively to some special means of ventilating it; nor that in the instructions

* Report, p. 38.

† Joseph Littlewood, p. 63; B. Byram, Esq., p. 66; Mr. Utley, p. 66.

‡ B. Byram, Esq., p. 66. § Report, p. 47. ‖ P. 47.

issued by the Secretary of State for the Home Department, this subject occupies a distinct head. But the more carefully and repeatedly I peruse these reports, or converse with practical men on this part of the subject, the more I find discrepancies, amounting almost to confusion, as to what is now attempted, what is now accomplished, or what can at any time hereafter be hoped for. And the difference in the accounts on this head does not appear to depend on real differences between the mines themselves, so much as on variety of belief, or of modes of expressing belief, as to what now takes place. On the whole, the following expressions seem to convey the general opinion—"Sweeping the outskirts of one or two gobs."* "Coursing by the side" of the goaf.† "Passing round it."‡ "This current takes its way sluggishly through the lower parts of the goaf, or moves round the outside of it."§ "All that oozes from it into the workings is removed by the air courses."‖ But if there be any truth in the description given by Faraday and Lyall, of the space above a goaf as an inverted basin of inflammable mixture, whatever difficulties may attend their plan, or any other, for its separate ventilation, only on effecting this depends our deliverance from the greatest danger of all. Many passages in these reports, too long to quote, confirm the accuracy of their description, and I might say of my own. Their statements are not matter of opinion, but of the most undeniable calculation, as to many thousand cubic feet of explosive mixture issuing at one spot, upon a sinking of the barometer, of only one-tenth of an inch, or a fall of roof, lowering the upper edge of the goaf basin only three inches. They understate their case, for they suppose only one-tenth. The fall

* Coppull, p. 48. † Oaks, p. 57. ‡ Oaks, p. 57.

§ Farady and Lyall, Phil. Mag., p. 23. ‖ P. 57.

within a short time is often greater; and they take for a basis the Haswell goaf of thirteen acres, instead of the Felling, or the Jarrow Bensham seam, of 100 acres each. More than all, they assume the goaf mixture to be itself simply explosive, instead of being capable of rendering explosive many times its own bulk of previously pure air. This latter must more often be the case.

Keeping in view these facts, to "sweep the sides" of a goaf, or carry off what "oozes" from it, may carry an explosive mixture to naked lights at a distance from the goaf, but cannot prevent its formation, or its discharge. And to pick up every stone from the floor, as held out at Jarrow, incomprehensible as it seems—or to pierce the fault bounding the south waste at the Oaks—would leave the goaf cavities untouched. It is not clear at what levels the "permanent windways over the gobs" at Risca* are placed. If they are at the top of the highest cavity in that weak shale, the first part of Faraday and Lyall's recommendation is effected; it is, therefore, practicable. What is further wanting is to carry off the goaf air separately, instead of into the air courses of the workings. If they are only at the level of the roof before its breaking down, they also leave the hollows untouched. Sir H. De la Beche and Mr. Smyth say, "The height to which it (the gas) might be mixed with the broken roof could not be estimated."†

As to sinking of the surface, I have heard persons engaged in coal mining converse on this matter as if, very soon after the last procurable coal was extracted, and the "juds" drawn, the roof settled down quietly, evenly, and entirely from the top, to meet the floor, or creeps in it,

* Report, p. 57.

† And Faraday and Lyall (p. 20)—"How high the heap of broken strata, and the vaults inclosing it, extend, is not known in a large goaf, nor, as far as we are aware, even in a small one."

until the whole cavity is filled at the expense of the surface. Now I know that the instances of subsidence are very curious, but their being objects of curiosity implies some degree of rarity. I believe the complete filling up of old mines from this sinking process to be slow, irregular, and by no means to be depended on. This is implied also in several passages of these reports.

We have the river Sirhowey flowing over the works of Risca colliery. Indeed, among the many mines of all kinds carried under rivers or the sea, I remember but two or three instances of flooding by irruption of water from the surface.

I have omitted many interesting particulars, out of regard to your time, and to the authors of other papers. The printed documents will well repay the attention of persons of all ranks engaged in such undertakings. It would be a great mistake to suppose that the appointment of inspectors in the least degree relieves proprietors or managers of collieries from responsibility, or that it will at all cramp their own exertions for the preservation of their property, and of the lives of their workmen. The intention is, and if all classes concerned do their duty, the effect will be, to assist, as well as to stimulate, individual exertion. It is much to be hoped that from the time when the inspectors about to be appointed commence their operations, they will collect, record, and publish all such particulars of every explosion of mark as may furnish to themselves, other inspectors, proprietors, and agents of every description and name, the means of learning from them what to adopt and what to avoid, for the purpose of preventing, or rendering less destructive, these calamities.

I do not deem it needful to dwell long upon that undeniable and prominent source of danger—the carelessness or fool-hardiness of workmen. We find from the evidence

in several of the cases that they have strong objection to lamps, on account of their giving less light than candles.* Mr. Green, at Risca, says, "I have much trouble with the men respecting the use of the lamp. I have frequently been obliged to make them an allowance, either by putting a boy with them, or by giving them an extra price, when they were asked to use the lamp. *I have many times seen colliers with the safety lamp hung up by them burning, and they working with a naked candle, also burning, close to each other.*"† Nowhere did the workmen complain that they could not readily obtain lamps, but wherever the subject is mentioned, we find unwillingness to use them; and this, although, as the evidence implies, the cost of lamps, oil, and wick was borne by the masters, but that of candles by the men themselves.‡

Mr. Smyth says, "Men have been known to dash to pieces Upton and Roberts's lamp, on account of its weight.§ As candles could always be smuggled into the pits, there appears little prospect of effecting, by compulsion, the general use of any safety lamp. Unhappily, the case of a coal mine, so far as explosions are concerned, differs from that of a ship, and from many other dangerous occupations, in that the care of the most prudent does not secure even themselves, while the carelessness of one may at any time involve the whole in destruction. At Jarrow, and at the Oaks, the immediate cause of explosion seems to have been, in each instance, a man going with a candle into a part of the waste notoriously unsafe; at the Oaks, too, after being repeatedly warned against it. The story is familiar to many, of Dr. Faraday, during his inspection of the Haswell mine, after the accident there, asking of the group of smokers among whom he was resting, "Where

* John Bainbridge, p. 20, at Jarrow.

† Report, 1847. ‡ P. 54. § P. 47.

is your powder?" and being startled from his rest by the answer, "You are sitting upon it."

The remedies for insensibility to their own preservation, and that of their fellows, must be chiefly of a moral nature, and must be very slow in their operation, requiring generations to produce much effect. Mr. Tremenheere shows, in a passage too long to quote, how, without compulsion, a taste for some degree of mental cultivation may be induced, and some degree of intellectual advance imparted. We must make great allowance for the very low state of degradation, and the almost complete isolation from the rest of the community, in which, a century ago, colliers were sunk. In Scotland they were, by law, a kind of serfs. On the Tyne, at Kingswood, and in Staffordshire, they were wholly a distinct class—the amusement, the wonder, and the terror of their neighbourhoods. I have lived near coal mines for forty years; I believe it is neither fancy, nor use alone, but some degree of approach towards the habits and manners of other workmen, which renders it far less easy to me to distinguish at a glance a collier, whether man or boy, by his dress, gait, and features. However this may be, they are human beings; and those masters and managers who, not only by liberal wages, but by kindness, and the evidence of their own concern for the men's safety, attach these to them, will have a security for the observance of needful regulations, stronger and more constant than fines, or sudden and angry dismissal.

I concur with most of the conclusions to which the different scientific reporters have come. I need not recapitulate them at any length. More shafts; separate and, so far as may be, distant shafts; scaling fresh air to the furnace; effectual ventilation of goafs, if possible; increased use of lamps; increased care and intelligence among the workmen, especially the firemen, wastemen, &c. The evils

TABULAR VIEW OF SOME RECORDED EXPLOSIONS.

Seam or Pit, Place, and County.	Date.	Reported on by	No. in Pit.	No. Killed.	Character as to Gas.	Condition on that Day.	Character of Ventilation.	Air. cubic feet ℔ min.	Speed of Air current	Longest Distance traversed by the Air.	Kind of Lights used.	Apparent or supposed Cause of Explosion.	Improvements recommended or suggested in the Reports.	Statements as to Waste of Candles.	Former Explosions in same Pit.
Little Pit, Haswell, Durham.	Sept. 1844	Dr. Faraday and Sir C. Lyall.	...	95	"Less fiery than many." "As little as any in that part of England."	Good.	"Most admirable."	25,400	...	...	Lamps only.	Fall of roof on a lamp in drawing Jud.	Drawing off the gas from the goaf, separate from the general ventilation.	"Almost impossible to keep a candle lighted in the windways."	...
Lower Seam, Jarrow, Durham.	Aug. 1845	Dr. Playfair.	37* 41†	36* 5†	Not reputed very fiery, but quantity of gas very considerable.	"No blower or other unusual escape observed."	"Sufficient." (Dr. P.) "Very good." (Pitmen.)	40,000	9 ft. per second in shaft	...	Candles in the Low Main	.....	Additional shafts. Use of lamps in the "Low Seam."	Great complaints of "sweating."	6 in 28 years.
Black Vein, Risca, Monmouth.	Jan. 1846	Sir H. De la Beche and Mr. W. W. Smyth.	150	35	"As a whole, much gas." "Very fiery." (Pitman.)	"Examined that morning; no gas observed."	Sufficient air; the plan in a "transition state."	Never observed.	...	1½ mile	½ lamps. ½ candles.	Man going into goaf with a candle.	An additional shaft and other arrangements for ventilation, as then in progress.	Some "worked with lamps because they could not keep a candle lighted," &c.	...
Rounds Green Oldbury, Warwick.	Jan. 1847	Mr. W. W. Smyth.	...	25	.....	.....	"No efficient system of ventilation can be said to exist."	...	...	...	Candles, except doggy.	.....	A furnace, or else a tower or chimney. Air-ways larger, carried to the top of the coal, and kept in advance. More care as to stoppings.	.....	...
Burgh, Coppull, Lancaster.	Jan. 1847	Ditto.	...	9	"Quantity of gas rather large."	.....	.....	...	...	Nearly 4 miles	Candles, except fireman.	.....	Better upcast draught. Shorter total air-course. More care as to stoppings and doors. Brattices to be carried nearer to the faces of work. Obedience of men to rules.	.....	...
Ardsley Main or the Oaks, Barnsley, Yorksh.	Mar. 1847	Sir H. De la Beche and Mr. W. W. Smyth.	...	73	"Average quantity."	Not complained of.	.....	...	...	...	Chiefly candles.	Man going into goaf with a candle.	Shafts too near each other. (A third shaft intended.) Lamps alone to be used near the goaf. (Works ill laid out by former proprietors.)	"Would sweal away a candle in two or three minutes, any day," &c.	...
Darley Main, Barnsley, Yorksh.	Jan. 1849	Mr. Smyth, Mr. Tremenheere, and Newspaper.	101	75	"But little."	"No sulphur." "As usual."	Quantity of air too small.	Average 9,000	...	...	Candles.	High wind, interfering with ventilation.	Much more air. (Mode of working disapproved.) Air should be "scaled off" for furnace. Upcast shaft too shallow; a tower or stack wanted.	"The men grumbled bard at the air sweating the candles," &c.	3 in 2 years.
Oldham, Lancaster.	Oct. 1850	Newspaper.	35	15	Not stated; must have been large, since lamps only were used.	.....	.....	...	...	...	Lamps only.	Fall of roof on a lamp in the workings.	Obviously one or more additional shafts.	.....	...

* In Low Main. † In Bensham.

TABLE 2, CONTAINING CHIEFLY ENGINEERING PARTICULARS.

Place.	Shafts. No. and Distance.	Shafts. Diameter.	Depth.	Extent of Work.	Thickness of Seam.	Dip.		Throws.	Character and Hardness of Coal.	Roof.	Floor.	Statements as to Goaf.	Kind of Explosion.	Mode of Working.
			Yards.	Acres.	Feet.									
Haswell.	.....	.....	310	250	5¼	1 in 24	To S. E.	Almost free.	Seam very regular. Some gunpowder used.	Shale and sandstone; very secure.	Sandy clay; not apt to rise in creeps.	One of 13 acres.	Believed to have been two.	.....
Jarrow.	1 bratticed into 3.	14 feet.	390	...	5	1 in 32	To S.	Several slight.	Some gunpowder used.	.....	.....	Said to be kept clear.	Two; the last worst.	.....
Risca.	2—24 feet.	Down 9; Up 16 by 10.	146	...	6 to 18 Aver. 9	Curved; 1 in 2 to 1 in 6	To N.W.	Some.	Very hard. Gunpowder said to be necessary.	Carbonaceous sbale; not strong.	Soft.	Wind-ways over them.	Small in proportion to damage done.	.....
Oldbury.	2—a few yards; No furnace.	7 feet.	180	Small at one time.	30	Irregular.	To S.W.	Very many.	Much shattered and injured. The height forbids the use of lamps.	Tender bind; in some parts sandstone.	.....	Walled off.	.....	In "sides of work."
Coppull.	2 near; 1 at considerable distance.	Down 12; Up 7.	154 140	200	5	7	...	Very many.	Gunpowder said to be necessary.	Carbonaceous shale; tolerably secure.	.....	"The air sweeps the outskirts."	.....	.....
The Oaks.	2—9 feet.	Down 9 to 10¾; Up 9.	283	...	8	11 and 14	To E.	1 Up, 17 ft. to W.	.....	Shale; readily breaking away.	.....	"Air-courses by the side." "Air passes round it."	.....	Long, or Shropshire.
Darley.	3	2 Down, 9½; 1 Up, 9½.	131 75	120	8	.....	...	One principal.	.....	.....	.....	.....	Small and slow.	Intermediate; between post and stall and long wall.
Oldham.	1 bratticed into 2.	.....	221	Small.	4¾	.....	To S.	.....	.....	.....	.....	.....	.....	.....

Seam or Pit, Place, and County.	Date.	Reported on by	No. in Pit.	No. Killed.	Character as to Gas.
Little Pit, Haswell, Durham.	Sept. 1844	Dr. Faraday and Sir C. Lyall.	...	95	"Less fiery than many." "As little as any in that part of England."
Lower Seam, Jarrow, Durham.	Aug. 1845	Dr. Playfair.	37* 41†	36* 5†	Not reputed very fiery, but quantity of gas very considerable.
Black Vein, Risca, Monmouth.	Jan. 1846	Sir H. De la Beche and Mr. W. W. Smyth.	150	35	"As a whole, much gas." "Very fiery." (Pitman.)
Rounds Green Oldbury, Warwick.	Jan. 1847	Mr. W. W. Smyth.	...	25	.....
Burgh, Coppull, Lancaster.	Jan. 1847	Ditto.	...	9	"Quantity of gas rather large."
Ardsley Main or the Oaks, Barnsley, Yorksh.	Mar. 1847	Sir H. De la Beche and Mr. W. W. Smyth.	...	73	"Average quantity."
Darley Main, Barnsley, Yorksh.	Jan. 1849	Mr. Smyth, Mr. Tremenheere, and Newspaper.	101	75	"But little."
Oldham, Lancaster.	Oct. 1850	Newspaper.	35	15	Not stated; must have been large, since lamps only were used.

TAB

Place.	Shafts.		Depth.	Extent of Work.	Thickness of Seam.	
	No. and Distance.	Diameter.	Yards.	Acres.	Feet.	
Haswell.	.....	.....	310	250	$5\frac{1}{4}$	1
Jarrow.	1 bratticed into 3.	14 feet.	390	...	5	1
Risca.	2—24 feet.	Down 9; Up 16 by 10.	146	...	6 to 18 Aver. 9	C 1 in 2
Oldbury.	2—a few yards; No furnace.	7 feet.	180	Small at one time.	30	Irr
Coppull.	2 near; 1 at considerable distance.	Down 12; Up 7.	154 140	200	5	
The Oaks.	2—9 feet.	Down 9 to $10\frac{3}{4}$; Up 9.	283	...	8	11
Darley.	3	2 Down, $9\frac{1}{2}$; 1 Up, $9\frac{1}{2}$.	131 75	120	8	st
Oldham.	1 bratticed into 2.	.....	221	Small.	$4\frac{3}{4}$	

attendant on blasting; the use of firemen understanding and observing the barometer; the error of supposing the wastes to fill up rapidly and entirely by subsidence; and, most of all, the hopelessness of increasing the speed of the air current, where candles are used, much beyond its present rate;—these are among the considerations which either have not occurred to those reporters, or have impressed me more strongly than they appear to have done these writers.

I end as I began; all that I have here collected and compared, all besides that I have read, all that I hear, even from those who advocate reliance on some single means of defence, convinces me that a single means is not enough. It will not do to trust alone to a strong current of air, to the superiority of "long" methods of working over "short," or "short" over "long," to the use of lamps, to frequent and minute inspection, or to regulations for workmen. Let each, or any, be observed; but let no other helps to safety, which are applicable to the situation and circumstances be neglected.

ON THE EXISTENCE OF FOUR CRYSTALLINE SPECIES OF CARBON. BY H. C. SORBY, ESQ., F.G.S.

The four species of carbon are diamond, graphite, hard coke, and charcoal. They have long been distinguished practically; and the object of the present paper is to show their various crystalline forms and mutual relationship.

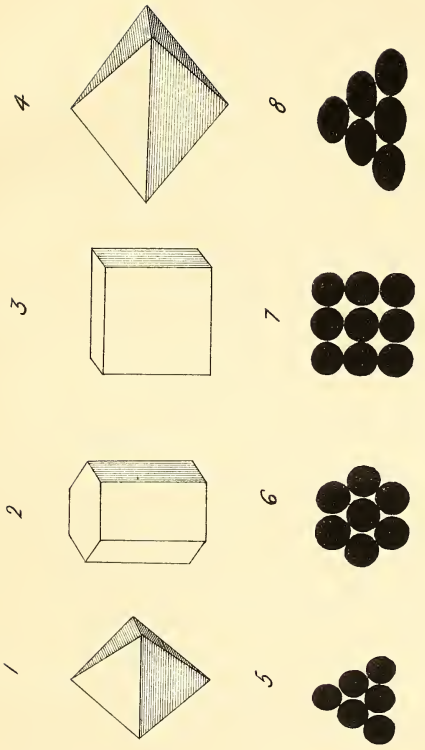
In the first place we have diamond, which is well known to be crystallized in the regular system, and to have a specific gravity of about 3.52.

Secondly, we have graphite or plumbago, which is found crystallized in regular hexagonal prisms, and has a specific gravity of about 2.18.

But besides these I find that there are two other crystalline species, viz., hard coke, which, like diamond, belongs to the regular system, though it has very different properties and specific gravity, viz., about 1.89; and anthracite or charcoal, crystallized in the square prismatic system, and having a specific gravity of 1.77. These various forms are shown in *Figs.* 1, 2, 3, and 4, *Plate II.*

The manner in which I have been able to ascertain the crystalline form of coke, anthracite, and charcoal, is thus:— I bruise a portion in a mortar, along with soft chalk, which protects the fragments from injury, and then dissolve away the chalk with acid, and wash the powder left; then spreading some of it on a piece of the thin glass used for microscope object covers, and drying it, I examine it through the glass with a magnifying power of about 400 linear, and measure the angles by means of the doubly-refracting goniometer, invented by Dr. Leeson. By this means, and using proper precautions, the angles of a fragment of $\frac{1}{4000}$th of an inch in diameter, or even less, can be measured within a small fraction of a degree. Of course by this method one can only ascertain the magnitude of the angles which occur on one plane; but by measuring those of various fragments, one can, by calculation, build up from them the primary form from which they are derived.

It is not every sample of coke which shows the crystalline structure well. Those which are highly vesicular do not show it at all distinctly; but if we take a dense, highly-metallic-looking specimen, although the particles which have good angles well placed for measurement are rare, yet still sufficient can be found by a patient examination to show that it belongs to the regular system. The angles which are good and distinct are, I may say, invariably found to be of 90°, 60°, 30°, 45°, and $70\frac{1}{2}$°, all of which are related, in a very simple manner, to the regular system; and I would


4
8
3
7
2
6
1
5

remark, that the angles found by careful measurement agree with those calculated, as near as could be expected from the nature of the case. The angles found in anthracite are $80\frac{1}{2}°$, $99\frac{1}{2}°$, 90°, $40\frac{1}{4}°$, $49\frac{3}{4}°$, 62°, 118°, 31°, 59°, 45°, $22\frac{1}{2}°$, $67\frac{1}{2}°$, $16\frac{1}{2}°$, $74\frac{1}{4}°$, $52\frac{3}{4}°$, and some others; the manner of derivation of which, and their mutual relationship, would require too much explanation. They are all, however, easily shown to be readily derivable from a square prism, which has its axes related as 5, 5, and 3, and which is not only cleavable with relation to those values of the axes, but also to the axes 5 and 5, and four times the axis 3; and that it is thus cleavable many fragments prove very clearly. I would also remark, that careful measurements agree, within a few minutes, with the angles calculated on these suppositions. Although charcoal is unfavourable for such an examination, yet I have found in it angles which agree with those of anthracite, as near as could be told.

Carbon is, therefore, crystallized in three different forms, viz., the regular and square prismatic systems, and in hexagonal prisms: and what is remarkable, (in fact, it is the only case of the kind known), it occurs in two perfectly distinct conditions, with different volumes, viz., diamond and coke, in one system of crystallization, the regular; making, therefore, four distinct species.

I will now explain what I believe to be the mutual relationships of these four species, and afterwards show in what manner their properties differ.

In order to determine the specific gravity of graphite, coke, anthracite, charcoal, and lamp-black, with sufficient accuracy, it is necessary to have them in a state of very fine powder; and in the case of charcoal it is absolutely essential to boil it first in acid, to dissolve away the glaze of ashes. I first heat the powder to dull redness, without access of air, and then thoroughly well boil it in water in a bulb of glass,

and fill it full of boiled water, and cork it. By this means the water, being deprived of air, when cold, dissolves any that may be amongst the powder. It is then weighed in water, and after that dried in the bulb, from which the weight in air is told. It is then burned to ashes, and their amount, and also their weight in water, ascertained; and these, being deducted from the weight of the substance in air and water, we are able to calculate its specific gravity, free from the effects of the ashes. In this manner I find the specific gravity of the different varieties of carbon to be thus, which are mean results of several experiments, the details of which it is needless to particularize here:—Graphite 2.177, hard coke 1.891, lamp-black 1.774, anthracite 1.760, and charcoal 1.784. I have not examined the specific gravity of diamond, but the mean of many statements is 3.521.

Now I have ascertained the following remarkable fact with regard to these specific gravities. If we assume the ultimate atoms to be spheres in the regular system and the hexagonal prism, and spheroids in the square prismatic, and calculate mathematically the relative specific gravities that we ought to have, assuming them to be arranged in the manner indicated by the crystalline form and cleavage, we find that the whole of the four species of carbon are brought about by its existing in ultimate atoms, having the relative volumes of $\frac{1}{2}$, $\frac{1}{3}$, and $\frac{1}{4}$.

In the first place, beginning with the square prismatic system, the mean specific gravity is 1.773, and this, multiplied by 2, gives 3.546 for the specific gravity of diamond; assuming the atoms to have their axes of the same relative magnitude as those of the two crystalline forms, and arranged similarly, that is to say, in the case of diamond, spheres, and in the other spheroids, with axes in the ratio of 3 and 5, as shown in *Figs.* 1, 5, 4, and 8. The mean of many statements of the specific gravity of diamond, which vary from 3.48 to 3.6, is 3.521, which differs from the calculated spe-

cific gravity by only $\frac{1}{140}$th. The volume of these two forms is, therefore, as $\frac{1}{2}$ to $\frac{1}{4}$.

Suppose the spherical atoms of diamond to be arranged as an octahedron, *Fig.* 1, in that case they would be placed with relation to one another on each equilateral triangular face, in the manner shown in *Fig.* 5; and such an arrangement agrees perfectly with the well known cleavage of this mineral. Also let us suppose the atoms of coke to be spheres, but arranged as a cube, *Fig.* 3, in which case they would be related to one another on each of its square faces, as shown in *Fig.* 7. On these suppositions it is easily shown mathematically that the relative volumes would be as $1 : \sqrt{2}$; and calculating on this supposition, and also that the volumes of the ultimate spheres in coke are to those in diamond as $\frac{1}{3} : \frac{1}{4}$, we should have for the specific gravity of coke, by calculation, 1.880. Four very careful experiments, which only differed by $\frac{1}{600}$th from one another, gave me a mean specific gravity of 1.891, which only differs from that calculated by $\frac{1}{180}$th.

Again, supposing the spheres of coke and graphite to be of the same magnitude, but in the latter arranged as an hexagonal prism, *Fig.* 2, in that case they would be related to one another on the terminal planes, in the manner shown by *Fig.* 6, and rectangularly in the direction perpendicularly to them, a structure indicated by the crystalline form and cleavage. If such be supposed, it is easily shown that their relative specific gravities should be to one another as $\sqrt{3} : 2$; and this gives for the specific gravity of graphite, by calculation, 2.172. The mean of six careful experiments gave me 2.177, which only differs from that calculated by $\frac{1}{400}$th.

I think, therefore, that these calculated results agree so very well with those found by experiment, that I am entitled to conclude, as I before said, that the four species of carbon are caused by its existing in atoms of the relative volumes of $\frac{1}{2}$, $\frac{1}{3}$, and $\frac{1}{4}$, arranged in such a manner as the form and

structure of the different crystals indicate; and what makes this fact very interesting is, that there is no case known at all similar, with respect to any other elementary body.

There is one fact of very great theoretical importance connected with these relative volumes, viz., their specific heat. In the case of charcoal and diamond I had predicted a considerable time before I knew that it was so, that their relative specific heats would be as 2 : 1; and such is really the case. Moreover, the specific heat of coke and graphite is the same, whereas it is to that of diamond as $\frac{1}{3} : \frac{1}{4}$, and to that of charcoal as $\frac{1}{3} : \frac{1}{2}$; whence it will be seen that the specific heat of the four species of carbon is in precisely the same ratio as the volume of their atoms; or in other words, the capacity for heat of the atoms of carbon, when of different volumes, varies in the same ratio with their magnitudes, which I believe to be an entirely new law as regards an elementary body. To render these facts more apparent I subjoin the following table, which is calculated from the found mean specific gravity and heat of the square prismatic form:

	Square Prismatic form	Coke.	Graphite.	Diamond.
Specific gravity—				
Theory	$2 \times \sqrt{2}$	3×1	$3 \times \sqrt{\frac{2}{3}}$	$4 \times \sqrt{2}$
	1.773	1.880	2.172	3.546
Experiment.....	1.773	1.891	2.177	3.521
Comparison of theory and experiment ...	...	$\frac{1}{180}$ too little	$\frac{1}{400}$ too little	$\frac{1}{140}$ toomuch
Relative volume of atoms	$\frac{1}{2}$	$\frac{1}{3}$	$\frac{1}{3}$	$\frac{1}{4}$
Specific heat—				
Theory	$\frac{1}{2}$	$\frac{1}{3}$	$\frac{1}{3}$	$\frac{1}{4}$
	.2714	.1809	.1809	.1357
Experiment.....	.2714	.2028	.2019	.1331
Comparison of theory and experiment ...	...	$\frac{1}{9}$ too little	$\frac{1}{9}$ too little	$\frac{1}{50}$ too much

NOTE.—In this table, in the upper line of figures, the number on the left hand side of the × indicates the inverse volume of the atoms; and that on the right the correction required for difference in crystalline form and arrangement of the atoms.

These specific heats are not from my experiments, but are the mean of those given in Gmelin's Hand-book of Chemistry; and though they differ considerably from theory, yet to any one acquainted with the nature of the experiments, and the usual great difference, as determined by various authors, it will not appear anything more than what might result from errors of observation.

If we take into consideration only the specific gravities, this law is not at all apparent; for, as I have shown, the specific gravity is not a simple function of the volume of the atoms, but a compound one of it and of the crystalline form and arrangement of the particles; whereas the specific heat is a simple function of the volume, and is in no way related to the crystalline form. So far as I am aware, this important distinction has been overlooked in discussing the relative specific gravities of various bodies.

Gmelin, in his Hand-book of Chemistry, in mentioning the relation of the specific heats of the various forms of carbon, says—"The capacity for heat of carbon in the form of diamond is one-fourth, in that of graphite one-third, and in that of charcoal one-half the ordinary amount. These exceptions cannot be explained away; we cannot triple, quadruple, nor even double the atomic weight of carbon without incurring great inconvenience." I, however, think that, from what I have shown to be the case in carbon, chemists will not insist on a constant, perfectly simple ratio between the specific heat and atomic weight of elementary bodies, since they may exist in different relative volumes, and thus much modify the relation of their specific heats.

The properties of these four species of carbon, as is well known, vary very remarkably, and it may, perhaps, be well to mention some of the leading differences between them.

In the first place, the distinction between diamond and charcoal is very great, and has long been a subject which

has excited much attention and surprise; and has hitherto, in my opinion, been referred to a very wrong cause, viz., mere molecular arrangement. Diamond is twice as dense, it is colourless and transparent; whereas the other is black and opaque. Its hardness is incomparably greater, and it is a non-conductor of electricity; whilst the other conducts tolerably well. It becomes positively electric by friction; whereas anthracite becomes negatively so.

The chief difference between diamond and coke is, that diamond is colourless and transparent, but coke is opaque and has a metallic lustre. It is a non-conductor of electricity, but coke conducts extremely well. It is also much harder than coke; for though coke cuts glass, and even scratches topaz, it will not scratch corundum. Diamond, as is well known, is converted by intense heat into the coke form of carbon.

Coke and graphite have both the same metallic lustre, and differ chiefly in hardness; graphite, when pure, being vastly softer. When coke is used for the electric light, that piece which is attached to the anode is softened, and partly converted into graphite, and its specific gravity is thereby increased; whereas that attached to the cathode is unaltered.

With regard to the square prismatic form of carbon, I would state that anthracite, charcoal, and lamp-black belong to it; and the chief difference between them and coke is, the want of the metallic lustre which the latter possesses. It is also very much harder; for anthracite will not scratch calcareous spar, whereas coke is inferior in hardness only to diamond and corundum. Coke is also a very much better conductor of electricity than they are. When exposed to a white heat, anthracite will cut glass like coke; and both it, charcoal, and lamp-black, when thus heated, conduct electricity vastly better than when only heated to dull redness. The specific heat of charcoal is also diminished. I therefore

think that the ultimate atoms are converted into the coke form, though they retain their former crystalline structure, and are therefore pseudomorphous. It is, however, remarkable that the specific gravity of anthracite is not increased by this treatment, when it is determined from fine powder.

One point of geological interest in this inquiry is, the fact that anthracite is found in the same condition as wood takes when exposed to a red heat, and not that produced by the action of heat on coal; for in the latter case we, I believe, invariably obtain the coke form. Why this should be so I will not venture to say, farther than that perhaps it may be owing to the manner of heating, joined to the great pressure which must usually have been in operation in the case of coal converted into anthracite. Moreover, it should appear that anthracite cannot have been exposed to a white heat, or else it would, in all probability, have been converted into pseudomorphous coke.

As to the manner in which anthracite has been converted into graphite, the fact of the piece of coke which is attached to the anode being converted into it, and not that to the cathode, would point not so much to the mere effect of heat, as to the combined action of a positively electric condition.

I should therefore conclude, from various circumstances, that diamond has been formed at a low temperature; the square prismatic or charcoal form at about a red heat; the coke form at a still higher temperature; and graphite at an intense heat, combined with the action of highly-developed positive electricity; and it is worthy of remark that the relation of their specific gravities, as calculated and as found by experiment, coincides with this view of the subject. I have compared them by calculation, (see table), supposing the charcoal form to be normal, and it will be observed that the specific gravity of diamond is lower, and those of coke and graphite higher, than indicated by theory; which agrees

perfectly with the supposition of its having been formed at a lower, and they at a higher, temperature.

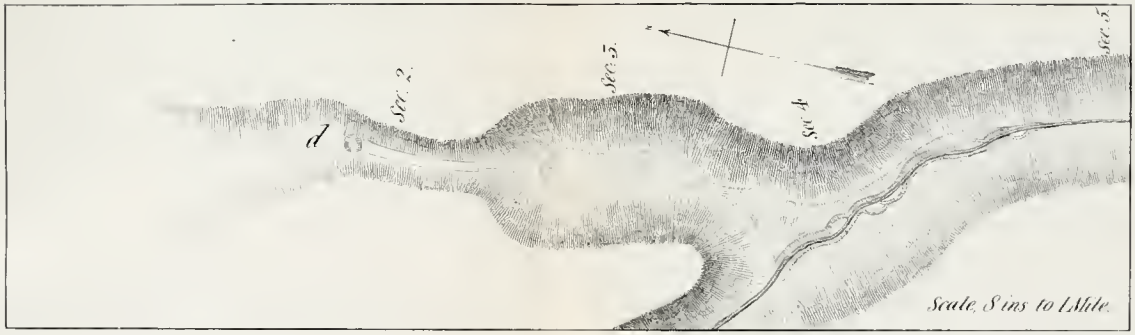
ON A NEW GAS STOVE. BY W. SYKES WARD, ESQ., LEEDS.

Mr. Ward stated that in his invention the stove is constructed in a vertical position, so as to expose a considerable surface for the absorption of heat from gas burners, and for the radiation of the heat; and that, from the flatness of construction, the apparatus occupies little space, not projecting into the room more than two or three inches, being thus productive of little inconvenience when out of use.

A plate of thin sheet iron is fitted into an ordinary fireplace, in the manner of a fire board, about two inches within the projection of the mantel-piece; about three inches in front of the back plate, a similar plate of sheet iron is secured by bolts; a third somewhat smaller plate of iron is placed about one inch from the second plate, and enclosed at the top, bottom, and sides, so as to form a chamber of about two to three feet square, and one inch in depth. Towards the bottom of the last plate a long aperture is cut, closed by a sliding plate, acting as a door for lighting the gas jets, and admitting a small quantity of air. A little below the aperture a pipe is introduced, in which are fixed three or more gas jets, either the ordinary small bat-wing burners, or burners with two or three holes, so that the flames may extend laterally, not coming into immediate contact with the iron. From the top of the enclosed chamber, a pipe, an inch and a-half in diameter, proceeds through the second and first plates into the chimney of the room.

The Author found that this apparatus was sufficient to raise the temperature of a moderate-sized apartment from five to ten degrees Fahrenheit, with a consumption of about three feet of gas per hour, costing about twopence for ten

PLAN AND SECTIONS OF YEDMAN DALE

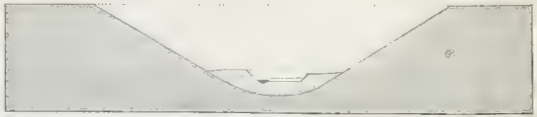


Section 1.

Section 2.

Section 3.

Section 4.



Section 5.

Scale of 100 Yds

Pulleyn & Hunt, 15th Leeds.

H C Sorby del

hours, and that it was particularly useful in warming a bedroom, where only a slight elevation of temperature was required, and perfectly free from the production of dirt or the slightest smell.

None of the products of combustion entered the room, and the ventilation was improved rather than impeded.

ON THE EXCAVATION OF THE VALLEYS IN THE TABULAR HILLS, AS SHOWN BY THE CONFIGURATION OF YEDMANDALE, NEAR SCARBOROUGH. BY H. C. SORBY, ESQ., F.G.S.

The Tabular Hills are a range of flat-topped hills, extending from Scarbro' westwards to near Thirsk. They are composed of coralline oolite, or calcareous grit, at the top, overlying Oxford clay, Kelloway's rock, cornbrash, and the upper sandstone and shales; and their peculiar configuration is very clearly traceable to this structure. The part to which I shall particularly refer is that near Scarbro'. Proceeding from the low flat tract of the vale of Pickering, the surface of the hills rises gradually to the north for some distance, and then the top becomes nearly level, having no undulations, and diversified only by deep-cut ravine-shaped valleys; the configuration and origin of which is the subject to which I wish to call your attention.

The nature of these valleys will be best understood from *Sec. 5*, *Plate III.*, where a portion of the level top will be seen on both sides, and the ravine-shaped valley cut sharply out, with the sides inclined at an angle of 30°.

We have, therefore, in these valleys a case in which it might with considerable propriety be concluded that they had been produced by the action of the streams now running in them, inasmuch as their form is the same as would be produced by such action, continued for a long period. We

have, however, in the case of Yedmandale, an association of circumstances that have, in a remarkable manner, preserved phenomena which clearly prove that they are not due to this agency, but to some other not now in action in the locality.

The configuration of that branch of this valley which exhibits these facts will be seen from the accompanying plan. The sides in every part, except those which I shall mention below, are inclined at an angle of 30°, and are quite straight from the top to the bottom, as shown by the sections. In the bottom we have a deposit of loose, coarse, not much water-worn detritus, of local origin, which is not deposited horizontally, but undulated, as indicated by the shading. It is owing to the coarse nature of this deposit, and to the fact of the neighbouring valley of the Derwent being about 300 feet deep, whilst Yedmandale is about 100 feet, that there never has been a brook in its northern arm; for the chief drainage passes into that deeper-cut valley, and what little runs into Yedmandale sokes away amongst the coarse detritus, without forming any brook. We have, however, a stream in the north-western portion of the valley, as will be seen from the plan; which, however, after passing along for some distance, is lost amongst the detritus, in a part below that shown in the plan. *Sec.* 3 is across the northern arm of the valley, in the part indicated on the plan, the steep sides and the deposit at the bottom being shown; the dotted line in this and the other sections indicating the position of the surface before the excavating took place. Passing northwards from this part, it will be seen that the end of the valley is round and amphitheatre-like; the central portion being inclined at an angle of 14°, and each way quickly becoming steeper. Ascending up the centre we come to a small valley with a flat bottom, as shown by *Sec.* 2; and though I could not ascertain it for certain, I think it contains no detritus. The steep sides of this upper valley round into those of the

lower one, and gradually vanish away to the north, as will be seen from the plan; the northern part being only a rounded depression, with no flat tract at the bottom. But the most remarkable feature is the small depression at the northern end of the flat tract at the bottom of the upper valley, as will be seen from the plan. *Sec.* 1 is in the direction of the length of the valley, along the bottom, from the point marked *d* on the plan, to a short distance in the lower valley. The above-mentioned depression is shown at *c* of *Sec.* 1; the part from *c* to *b* is the bottom of the upper valley, and from *b* to *a* is the inclination down the centre of the round end of the lower, with the detritus at the bottom. The brook coming down the north-western arm of the valley has cut out a small ravine, and formed some small alluvial tracts in the detritus at the bottom of the valley, as will be seen from the plan and *Sec.* 4; and as we proceed downwards it has removed the whole, and brought the valley into a state in which there is no evidence of its not having been wholly excavated by the brook, as shown by *Sec.* 5, where the lowest dotted line shows the original level of the detritus.

Taking all these facts into consideration, I think no one can imagine that any stream of water, such as now usually runs in such valleys, can have excavated Yedmandale; and from it I think we may safely infer the same of other similar ones in the locality, which, owing to circumstances, do not retain their original structure, but have been modified by the action of the brooks running in them. It is, however, not so clear what other action produced them; and though I do not wish to push theory farther than phenomena clearly lead, I must confess that a strong current from the north, when this part was below the level of the sea, offers a better explanation, in my opinion, than any other that I can suggest; and I would remark that, when on the spot, this view of the subject presented itself to my mind most strongly. The

part of all others which, I think, indicates such an origin, is the depression at the top of the small upper valley, which is precisely what a current coming from the north would produce there, as will be seen from *Sec.* 1: for a current coming in that direction, and passing the northern side of the depression, which is double the height of the southern, would have a tendency to scoop out such an excavation, in the same manner as we see in all waterfalls and eddies. Moreover, the large expansion of the valley agrees, I think, better with this supposition than with any other. Whether or no this current was one produced by sudden elevation of land to the north, and was directly connected with the erratic drift which is found in such large quantity in the neighbourhood, would be a problem of interest, but one which, I fear, would take up too much time to discuss. I might, however, remark, that I think it is more probable that it was excavated, and even laid dry to a certain extent, previously to the time when the chief part of the erratic drift was deposited; for, as far as I could ascertain, the detritus in this and similar valleys in the neighbourhood is entirely of local origin. On the contrary, the erratic drift appears to be confined chiefly, if not entirely, to a lower level, as though some portion of the higher lands had been above the level of the sea.

I will not, however, enter farther into this subject, for I am most willing to confess that I think there are many serious difficulties to contend with in adopting any theory whatever to explain the mutual connexion of all the phenomena associated together in the neighbourhood; though I am of opinion that the supposition of there having first been a wave of translation from the north, and subsequently drifting by means of ice-bergs, agrees with the whole phenomena better than any other.

ON FUEL—ITS NATURE, VALUE, AND APPLICATION. BY W. L. SIMPSON, ESQ., C.E., THURNSCOE HALL, DONCASTER.

Although England has derived great part of her national glory from the fact of her being situated near the centre of the earth's civilization, and from her insular position affording capabilities for commerce which the extreme richness of her soil enables her to originate and support, yet there is one condition of her political power, the absence of which has caused nations, otherwise similarly endowed, to fall far short of our own both in agricultural and manufacturing development.

This condition it is my object in this paper to bring in detail more prominently before you. I allude to the existence of large, almost illimitable, resources of *fuel*. Whether the processes of manufacture, the operations of agriculture, or the facilities for speedy transit of manufactured products from different parts of our own, or the most distant of foreign countries, are conceived to be the chief agents in the advancement of our national prosperity, in all and each we cannot but recognise the absolute necessity, everywhere so strikingly manifest, of an abundant and economical production of artificial heat. In nature, heat appears as an universally distributed vivifying and fructifying principle; and in art the elimination of force is almost equally dependent on its presence.

The following remarks will, however, illustrate more clearly the different applications of which it is capable. I propose first to consider the *nature* of the different fuels in common use; and, secondly, to ascertain the relative economic value of each, concluding with deducing from such values a few practical suggestions on improvements of which our present system is capable.

First, then, let us examine the nature of *Wood* as a fuel.

Wood, which is a vegetable substance, whose ultimate composition is carbon 36, hydrogen 22, oxygen 22, is composed of three distinct constituents—first, woody fibre; second, a compound of carbon, hydrogen, and oxygen, which forms the cellular tissue in which resides the sap; and, thirdly, water. The composition of dried wood may be taken (on the average of several analyses) as carbon 50, hydrogen 6, and oxygen 44. The woody fibre and the sap are supporters of combustion, which the remaining constituent, water, deteriorates. The quality of wood, therefore, as a combustible, depends on the quantity of water it contains; but it cannot be said that those woods which contain the greatest amount of carbon are the best for combustion, as the lighter woods, whose sap is of a more resinous nature, usually burn more readily, and cause a greater elimination of heat. Wood is but little used as a fuel in this country, its rapid combustion and the small amount of heat and flame it affords, having prevented its adoption, except in cases where the prevalence of extensive forests, as in America, have rendered it the most economical fuel. In the lowlands of Lincolnshire, however, the remains of the ancient forests cut down by the Romans during their invasions of England, which have been preserved by the antiseptic properties of the peat water, rich in protoxide of iron, form a local magazine of fuel which is sufficient, not only for fencing in the lands, but also very serviceable, from its easy combustion and the brilliancy of its flame, for the fires of the adjacent dwellings. The practical experiments recently made on the evaporative power of wood, give results as low as 3.097 and 3.086. But the difference between these and the figures given by other chemists arose, probably, from the smallness and dampness of the wood employed, and the necessity of a quick draft to carry off the large volume of smoke evolved.

We will next consider *Peat, or Turf*, as a fuel. Peat consists of decayed plants accumulated through a number of years, and compressed into a compact form by the weight of successive layers annually superimposed. When on a retentive soil, the flatness of the land or temporary obstructions prevent the outlet, and cause the consequent stagnation, of water, the area will soon be overgrown by water plants of all descriptions, as the sedges, rushes, algæ, mosses, &c. A species of moss, called spagnum, having the property of throwing up new shoots on its upper part, while its lower extremities are decaying, is the principal plant found in such situations. These plants, during the winter, die and fall to the ground, making room for another crop in the ensuing spring. The constant succession of this process produces a vast accumulation of decaying organic matter, which evolving carburetted hydrogen, (C_2 H_4) carbonic acid (CO_2), and olefiant gas (C_4 H_4), and other noxious gases, and taking up oxygen from the surrounding water and soil, assumes the form familiar to us by the name of peat. The superposition of Kimmeridge upon Oxford clay, without an intervening porous stratum, is the most favourable locality for its formation, and is well exemplified in Lincolnshire, where such an order of stratification actually exists. The nature of peat varies with its geological age, and may be divided into recent peat, exhibiting still the roots and stems of the plants from which it was formed, very porous, and of a light fibrous texture; and the older peat, reposing below this, in which the fibrous merges into an earthy texture, and of which the weight of a cubic foot varies from 50 to 80 lbs., while the former weighs only 4. The composition of peat varies extremely with its age, density, and the locality from which it is obtained. The general result of its analysis gives, carbon 57, hydrogen 6, oxygen 32, and 5 of ash. The

proportion of carbon is evidently greater than in woody fibre, the peat during its decay evolving its hydrogen and oxygen, and becoming gradually more rich in carbon. We will examine the nature of this change more in detail when we come to the consideration of the coal formations. Peat is extensively used as a fuel in Ireland and parts of Germany. In the latter country it is merely cut by the spade into the form of bricks, and dried, the surface soil and peat having been previously removed. In Holland, where the peat is an older formation, it is moulded and pressed into bricks by a process almost identical with the manufacture of bricks from clay. The objections to the use of peat as a fuel are its extreme lightness, porosity, and brittleness, together with the quantity of water and ash it contains. 100 lbs. of peat contain from 1 to 33 lbs. of incombustible ash, derived from the vegetable matter of which it is composed. In Ireland attempts have been made to render the peat more portable and solid by compression in powerful hydraulic presses. The peat thus subjected to pressure loses nearly one-third of its weight of water, and from the increased aggregation of the particles of carbon, contains, in a like bulk, a much larger proportion of combustible matter. But the economic value of the peat is not sufficiently improved to counterbalance the expense of so costly a process; as the peat is not even then sufficiently rich in carbon to admit of its general adoption, especially for steam purposes.

A patent has, however, been obtained by a Mr. Hill, for distilling dry peat, and collecting the pyroligneous spirit and tar. The tar is converted into pitch, which he mixes while hot with peat charcoal, and thus renders a very bulky, and, for many purposes, valueless article, one of great importance.

Peat Charcoal is prepared in two ways. First, in circular mounds formed by arranging from 700 to 1,000 brick-

shaped masses of peat in concentric layers round a central stake, with apertures to admit the air on all sides, the whole being covered with earth or charcoal dust. Or it is prepared in furnaces of masonry formed like large vertical cylinders, having apertures to admit the air, furnished with regulating valves at the sides. The objections to the use of peat, however, avail equally against the adoption of charcoal made from it, namely, its extremely loose, brittle character, and the quantity of ash it contains, the percentage of which, originally great in the peat before charring, becomes, of course, greater in the charcoal. Ordinary peat charcoal has been known to contain as much as 21 per cent. of ashes. The formation of patent fuel from it will, however, give additional consolidation to the charcoal, and the oleaginous compounds with which the mixture is effected impart a greater degree of combustibility to the mass.

We have hitherto been considering fuel of comparatively modern origin. But the present organic world has contributed but a small share to the support of artificial heat. The wants of man in this respect were as amply provided for by Providence during the primeval ages of the world as his other requirements, more apparently, but not less really, important.

The limits of this paper compel me to abstain from entering into the most interesting question of the ancient structure of our globe. Whether it was by the action of aqueous or igneous causes that the vast disturbances and transformations, the marks of which the crust of the earth now exhibits, were originally occasioned, is not now to be determined. But geology has demonstrated that the most marked and hidden changes of the climate and the position of the terrestrial portion of our planet have been amongst the most frequent phenomena of ancient times. The fossil remains, both in the animal and vegetable kingdoms, point

with undeniable clearness to the fact of the existence of a climate in the northern hemisphere differing widely from the present, and more analogous to that now existing in the tropics. The gigantic calamites, lepidodendra, sigillariæ, and stigmariæ, fed by the rich soils which no previous vegetation had impoverished, and fostered by the uniform warmth and humidity of the climate, formed at that time a mass of vegetable life of which even our tropical forests convey but a faint idea. In endeavouring to account for this fluctuation of climate, I have been led, after a careful study of the evidences hitherto adduced, to reject the theory of astronomical causes being a principal or even a minor agent. The mean amount of solar radiation being in the inverse ratio of the minor axis of the sun's orbit, would vary, of course, with the eccentricity of that orbit; but the lapse of years required before such eccentricity would be sufficiently great perceptibly to influence the climate of our planet, renders it difficult to conceive that geological changes of such magnitude could be effected by it.

It was formerly supposed that the plants of the carboniferous era were so closely allied to tropical genera, and so much larger in size than the corresponding tribes now inhabiting equatorial latitudes, as to imply an extremely hot as well as humid and equable climate. Recent investigations have, however, shown so wide a difference between past and existing species, that we must rather infer the existence of a climate more equable, but essentially different from, rather than analogous to, any existing climate. The evidence strongly points to the existence of a more uniform and temperate climate, rather characterised by the absence of any severe cold, than by the presence of extreme heat. I am not now going to discuss the probable cause of such a change of climate, but I would express a strong conviction that the elevation and depression of large tracts of

land, and a corresponding variation in the ratio of land to sea in the northern hemisphere, has been a principal agent, assisted, as it necessarily would be, by a consequent change in minor physico-geographical phenomena. The hypothesis that those vast masses of vegetable remains which form our coal fields have been transported from foreign climes by the overwhelming agency of large bodies of moving water, cannot, I think, be maintained, when the position of the flora, and the angular shape and large size of the particles of rock with which they are associated, are accurately observed. The rich supplies of carbonic acid from the atmosphere, and the fresh unimpaired soils of the period, formed the sustenance of vast forests which flourished for a long series of years with extraordinary vigour.

The districts, however, whose elevation above the sea level had introduced such remarkable modifications in the climate, rested on no secure foundation, and during subsequent disturbances seem to have been again submerged. The existence of the alternating freshwater beds of coal and strata, from the shells they contain, evidently marine, indicates that these convulsions were more gradual than the preceding elevations; and that while the depressions appear to have been local, and the area of water confined to the valleys, the mountains and higher lands projecting above the sea formed an ocean studded with islands. These islands, after the erosion of their sides had furnished material for the marine deposits alluded to, again, by a series of gradual elevations and depressions, assumed the form of a new and vast continent, whose soil, vegetation, and climate, had been transformed into our own. The ancient forests, thus imbedded in the marine deposits with which we now find them associated, underwent a process of eremacausis or decay, varying in its nature with the depth of

the superincumbent strata, and the corresponding presence or absence of the atmospheric oxygen. In the more recent tertiary strata, we find what is usually termed the *wood coal*, or *brown coal*, of Germany, during the formation of which the air has not been entirely excluded.

The essential change in the conversion of wood into brown coal seems to be the separation of carbonic acid from its elements, while a portion of hydrogen is removed by oxidation, owing to a limited access of air.

For if from wood, whose composition is	$C_{36}\ H_{22}\ O_{22}$
We take one atom of hydrogen, and three atoms CO_2	$C_3\ H\ O_6$
There will remain brown coal	$C_{33}\ H_{21}\ O_{16}$

The separation of carbonic acid is even now going on, as evidenced by the acidulous springs found in the neighbourhood of such beds, and by the presence of choke damp (carbonic acid) in the mines.

Mineral coal, however, which occurs in the secondary or primary formations, appears to have resulted from the long-continued decomposition of wood, or wood coal, without access of air; the separation of carbonic acid (CO_2), water (HO), and carburetted hydrogen (CH_2), being the result.

For if from wood ... . =	$C_{36} + H_{22} + O_{22}$
We take 9 equivs. (CO_2) ..... =	$C_9 + O_{18}$
	$C_{27} + H_{22} + O_4$
3 equivs. water ..... =	$H_3 + O_3$
	$C_{27} + H_{19} + O$
3 equivs. carburetted hydrogen ... =	$C_3 + H_6$
There remains mineral coal, whose composition is	$C_{24} + H_{13} + O$

Or if the composition of wood be taken, as it is by some chemists, as $C_{36}\ H_{44}\ O_{22}$, then the resulting coal will have

a composition of C_{24} H_{26} O. The rapid evolution of carburetted hydrogen in situations where its escape was previously impossible, has frequently caused the fearful explosions so often following the opening by the miners of cavities in the coal strata filled with carburetted hydrogen, liquified by pressure.

The older the formation of coal, the more is its composition analogous to pure carbon, till we arrive at last at the anthracitic beds, which, as I have before noticed, contain extremely little hydrogen or oxygen.

The coal formations may, therefore, be divided into three classes:—

1st. The coal formation, subdivided into the older (anthracite), and the younger coal formation (coal).

2nd. The secondary formations found in the neighbourhood of the chalk.

3rd. The tertiary formations, consisting of alternations of brown coal with freshwater limestone.

In the latter or tertiary formations the coal still retains all the forms of its vegetable origin, consisting generally of the flattened stems and leaves of ancient plants. It possesses no peculiar excellences as a fuel, yielding on an average from 3 to 7 per cent. of ashes, 60 to 70 per cent. of carbon, 4 to 6 of hydrogen, and 18 to 36 of oxygen. Its combustion is not attended with a good flame, and though much valued in Germany, where fuel is comparatively rare, would be considered as of very inferior quality in England.

Mineral coal, which we have next to consider, is so well known that I shall only remark, that it may be divided into fibre coal, appearing in thin layers, parallel with the plane of stratification, richer in carbon than the rest of the mass; pitch coal, associated with bituminous hydro-carbons; and slate coal, so named from its fissile nature.

A mixture of iron pyrites (sulphuret of iron Fe S) is frequently found in coal, and is very objectionable, from the action of its sulphur on the iron bars of furnaces, &c.

Coal yields from 1 to 30 per cent. of ashes, but usually less than that obtained from peat or brown coal. Its specific gravity is about 1.3 on the average. Anthracite, a common formation in the older series in Wales, is eminently homogeneous, and without impressions of plants, and consists almost entirely of carbon; the amount of its ash varies from 1 to 6 per cent.

Time will not admit of my describing now the varieties of fuel resulting from subsequent artificial preparation of these natural fuels, as coke, charcoal, &c. I cannot, however, pass over the subject of patent fuels. The brittle nature of our coal deposits, and the difficulty of combustion attending coal so mechanically divided as to exclude the free circulation of air, has caused the accumulation of vast heaps of small coal at the mouths of our coal pits. Several patents have recently been taken out proposing to convert this, by various processes, into a useful and economical fuel. The most extensively employed of these patent fuels is that of Mr. Wylam. A brief sketch of this fuel, with the results of experiments on its composition, will suffice to give a general idea of the whole.

He first distils *coal tar*, in a large iron retort, furnished with suitable condensers, into *naphtha*, *dead oil*, and *pitch*. The naphtha being the most volatile, passes over first, and is re-distilled at a temperature of 212° by steam, and after admixture with chloride of calcium (Ca Cl) or quick lime (Ca O), and a third distillation, becomes perfectly colourless and fit for sale. The dead oil obtained as the second product of distillation, is used extensively for the purpose of illuminating manufactories and other large works; as a solvent for pitch, thus forming a valuable varnish; and

lastly, by being burnt in long galleries, it forms large deposits of a most excellent lamp black. The residue, (pitch,) when cold, is ground, and mixed with small coal in the proportion of 1 to 4, and having been again heated, is cast into brick-shaped moulds, and subjected to an enormous pressure. The composition of one of these bricks was carbon 76.60, hydrogen 5.15, nitrogen 1.54, oxygen 9.63, ashes 7.08, in 100 parts; while another specimen more accurately analysed gave from 79 to 80 per cent. of carbon, and from 4 to 5 of ashes.

These patent fuels have many great advantages, and some qualities equally prejudicial. Their form, that of equal-sized cubical bricks, renders them well adapted for stowage; for although the specific gravity of patent fuel is lower than that of ordinary coal, from their shape and mechanical structure, there are few coals which could be stowed away in a smaller space per ton. When examined in the Admiralty coal investigation, the three patent fuels of Warlich, Wylam, and Bell, ranked among the highest result obtained, and in the order named. For the purpose of the steam navy, however, the dense black smoke they evolve during combustion is extremely inconvenient, betraying, as it does, the position of ships of war at a time when it is most important to preserve concealment. This, added to the fact that the bitumen they contain renders them liable to spontaneous combustion in hot climates, has hitherto prevented their meeting with the notice they deserve. Errors in their manufacture are frequent, as the mixture of bituminous or tarry matter with bituminous coal, instead of with anthracitic, as theory would point out; and in some varieties the addition of large quantities of lime, salt, clay, and other incombustible matters, has occasioned the production of so great an amount of ash as to prevent their application to any economic purpose.

Having, then, discussed briefly the nature of the different

fuels in common use, we are now in a position to examine the effects of heat upon them. All fuels are of very complex constitution, and it is a law of organic chemistry that the greater the complexity of a body, the easier is its decomposition effected. In bodies so compounded the effect of decomposition is usually a disturbance of the existing equilibrium of the elements, and their re-arrangement into new compounds. The nature of these compounds depends on the external circumstances with which the decomposition is attended, and on the presence or absence of oxygen. The action of heat upon fuel exhibits instances of decomposition of both kinds. The first process is a dry distillation of the elements of the coal without access to air. It is erroneous to suppose that the combustion of fuel is caused by the direct action of atmospheric oxygen on the elements themselves. The hydrogen and oxygen of the fuel, on the application of heat, abandon their former state of equilibrium, and form new products with the carbon, the superior affinity of which prevents the formation of more water (^{HO}) than the equivalent of oxygen and hydrogen in the coal admits. We have, therefore, first, the oxygen uniting with the hydrogen to form water, and then the excess of hydrogen (which there always is in coal above the quantity required to form water with the oxygen) unites partly with the nitrogen, and forms (^{NH_3}) ammonia, and partly with the carbon, whose equilibrium has been disturbed by the movements of the other elements; carburetted hydrogen ($^{C_2\ H_4}$) and olefiant gas ($^{C_4\ H_4}$) resulting. These binary compounds again acting chemically on each other, give rise to an endless series of ternary compounds of small economic value, of which the principal are acetic acid, pyroxylic or a wood spirit, naphtha, naphthaline, paraffin, picamar, kreosot, kopnomor, and pitacall. The second process is the combustion of these products of distillation, and it is this

which causes the evolution of heat, and the illuminating powers of the fuel. There is, however, no heat lost by the previous distillatory process, as, although there is an immense amount of latent heat absorbed in rendering these products volatile and gaseous, it is exactly counterbalanced by the quantity of heat eliminated by their subsequent combustion.

An augmented proportion of hydrogen in a fuel, accompanied, as it usually is, by a corresponding deficiency of oxygen, increases the number and quantity of the hydrocarbons evolved, and has frequently the effect of causing the fuel to cake into solid lumps, choking up the bars, and preventing a due supply of air to the fire; while, on the other hand, anthracite being, as we have seen, almost pure carbon, evolves so few hydrogenised products, as not to soil the whitewashed fireplaces and chimneys in which its combustion is effected.

Let us now pass on to consider the relative value of fuel. And first, let us endeavour to discover the principles governing the maximum theoretical value of each. To accomplish this we must take into consideration, first, the quantity of heat which a certain amount of fuel is capable of producing; and secondly, the time which is required for that object.

The experiments of Lavoisier and Laplace on this subject have warned us that it is the relative, and not the absolute, quantity of heat produced by different fuels which we must seek to ascertain. Attempts have been made to establish this by the aid of calorimeters, which, by registering the quantity of ice melted, or the difference in the temperature of water occasioned by the combustion of a given quantity of each fuel, rendered the calculation of their respective heating values easy. There are, however, other more convenient methods grounded on the law enun-

ciated by Laplace, that those quantities of a combustible body which require an equal amount of oxygen for combustion, evolve also an equal quantity of heat. The oxygen required in the combustion of a fuel thus becomes a standard by which its relative heating value can be deduced.

That greatest of modern chemists, Berthier, has founded upon this law a practical process as follows. A weighed quantity of the fuel very finely pulverised is intimately mixed with about 1,500 grains of litharge (oxide of lead, Pbo), and the whole introduced into an earthen crucible, the cover of which is luted down with fire clay, and, when dried, allowed to remain about fifteen minutes in an assay furnace, with a moderately strong fire. The combustible will reduce a certain quantity of the oxide of lead to metallic lead by uniting with its oxygen, in the same manner in which a body burns in atmospheric air by the abstraction of its oxygen. The number of equivalents of oxygen required to effect such combustion will, therefore, be accurately indicated by the number of equivalents of reduced metallic lead, which can be easily estimated from the weight of the button produced.

According to the experiments of Despretz, 1 equivalent of carbon is sufficient to heat 78.15 equivalents of water from 0° to 100° Centigrade; and as the same quantity of carbon requires 2.666 equivalents of oxygen, which taken from litharge leaves 34.5 equivalents of metallic lead, we have for the result, that every equivalent of lead that is reduced by an experiment with any kind of fuel, corresponds to $\frac{78.15}{34.5} = 2.265$ equivalents of water, which will be raised from 0° to 100°. But this method is merely approximate, and obviously, from the amount of hydrogen in the fuel above what is required to form water with the oxygen not being taken into consideration, very serious error may be

introduced. Practical experiments have shown that the determinations made with litharge are always (about one-ninth) short of the truth. An elementary organic analysis is a much more correct method of finding the calorific values.

The theoretical value of fuel, more particularly of coal, with reference to the number of pounds of water which 1 lb. of fuel will convert into steam being given by the formula $\left(\frac{C \times 13{,}268}{965.7}\right) \times \left(\frac{(H—h) \times 62{,}470}{965.7}\right) = x$, in which C is the quantity of carbon obtained from an analysis of the fuel in question, H the quantity of hydrogen, and h the amount of this hydrogen, which corresponds to the oxygen contained in the fuel, these, multiplied by their heating powers, 13,268 and 62,470 respectively, (obtained from the accurate experiments of Dulong), and divided by the latent heat of steam, indicate the number of lbs. of water that can be converted into steam by 1 lb. of coal. It is, however, frequently more convenient to have this number converted into an expression of mechanical force, which can be accomplished thus. Let w represent the quantity of water of which (u) lbs. are evaporated by 1 lb. of coal, then $w\ u \times 965.7 \times 782 = x$ the actual duty obtained by a lb. of coal, namely, the number of lbs. of water raised by it to the height of one foot. This formula is founded on the fact that (u) lbs. of water, multiplied by 965.7, or the co-efficient for the latent heat of steam at 212° as obtained by Regnault, indicates the number of lbs. of water which would be raised 1° Fahrenheit, and the number 782 arises from experiments on the mechanical force denoted by the elevation of 1 lb. of water 1° Fahrenheit: that force, according to the careful experiments of Mr. Joule, on the friction of oil, water, and mercury, being equal to 782 lbs. raised to the height of one foot.

But in ascertaining the relative value of a fuel, we must

take into consideration the specific objects for which the fuel is to be employed. A fuel pre-eminently calculated for steam purposes may be one of the least so for other equally extensive applications. The relative evaporative values of different fuels, or their power of producing steam, are, however, too important to be passed over without special and separate notice. The qualities of fuels, with reference to this point, are so remarkably varied, and the necessary excellences so seldom united in one species, that it becomes difficult to deduce any definite general results. The characteristics essential for steam purposes, having especial reference to the steam navy, may be stated as follows:—

1. That the fuel should burn so that steam may be raised in a short period, if this be desired; in other words, it should be able to produce quick action.

2. It should possess high evaporative power; that is, be capable of converting much water into steam with a small consumption of coal.

3. It should not be bituminous, lest the particles of coal should become cemented together into tough and infrangible cakes, obstructing the passage of air through the bars, and its free circulation through the mass of combustible.

4. It should possess considerable cohesion of its particles, so that it may not be broken into too small fragments by the constant attrition which it may experience in the vessel.

5. It should combine a considerable density with such mechanical structure that it may be easily stowed away in a small space, a condition which, in coals of equal evaporative values, often involves a difference of more than 20 per cent.

6. It should be free from any considerable quantity of sulphur, and should not progressively decay; both of which circumstances render it liable to spontaneous combustion.

7. It should not contain a large per centage of ash.

The best coal for steam purposes should unite all these qualities in itself, but a coal possessing the first and not the fifth may, for ordinary purposes, be equally efficacious.

For upwards of twelve months I was enabled almost daily to inspect the experiments of the Admiralty coal investigation, conducted by Dr. Playfair and Sir H. De la Beche. It would, however, be tedious to enter here into the details of these extensive experiments, but I think it may be profitable to state that the relative evaporative value of each coal was tested by three independent experiments, beneath a steam boiler specially erected for the purpose, and furnished with every contrivance for obviating the perturbating influence of external causes.

The following particulars were attended to and results obtained. For each coal was marked, the time required to get up the steam; the weight of wood therein employed; the initial temperature of the water in the boiler and the tanks; the state of the barometer and dewpoint; as well as the extremes of external and internal temperature; the area of damper open; weight of coals consumed; weight of ashes, cinder, and soot left, and their respective proportions of combustible matter and clinker; weight of water evaporated from 212° by the whole, and each pound of coal per foot of grate surface; together with the duration of the experiment, the specific gravity, mean weight of a cubic foot, cohesive force, and economic weight of each variety of coal operated on.

The general result of these experiments, which were conducted with scrupulous accuracy, was strongly corroborative of the results obtained theoretically; and the following formula, by which the practical results were calculated, may be taken as of very considerable accuracy for coal:—

Let W = the weight of water let down from the tanks during the experiment.

w = weight of water in boiler at the commencement of the experiment.

w′ = weight of water in boiler at the close of the experiment.

q = weight of wood used for lighting the fire.

l = co-efficient of latent heat of steam = 965.7.

t = quantity of heat necessary to raise the water in tanks from its mean temperature to that at which it is evaporated.

t′ = quantity of heat necessary to raise the water in the boiler from the initial to the final temperature.

t″ = quantity of heat necessary to raise water at the temperature of the tanks to the final temperature of the boiler.

P = weight of coal consumed during experiment.

E = the co-efficient of evaporative power of wood.

E′ = co-efficient of the evaporative power of coal.

Then

$$\frac{(W - Eq + w - w')\,l + (W + w - w')\,t + wt' + (w' - w)\,t''}{P\,l} = E'.$$

The relative value of the different fuels, as shown by these experiments, need not be here enumerated. The average result of thirty experiments on coal and patent fuel gave 8.69 as the number of lbs. of water converted into steam by 1 lb. of coal. This quantity, it may be remarked, is far below the theoretical result. There are at present no authentic experiments on other species of fuels, with which these may be compared. I have, however, taken an average of the theoretical value of various fuels, as obtained from the litharge experiments, which will be sufficient to exhibit their relative economic importance.

For different kinds of wood, lbs. of lead reduced by 1 lb. of dried wood	13.4
Do. do. do., wood containing 90 p. c. of Ho.	13.9
No. of lbs. of lead reduced by 1 lb. of wood charcoal	30.9
Ditto varieties of peat (foreign)	14.7

No. of lbs. of	peat charcoal (foreign)	21.1
Ditto	brown coal.....	19.9
Ditto	mineral coal, as given by Berthier..	26.8
Ditto	mineral coal, as by Mr. Phillips ...	29.8
Ditto	coke	26.2
Ditto	anthracite	29.6

The preceding table will enable us to form a tolerably accurate idea of the relative heating powers of different fuels. I must, however, reiterate the caution that these figures exhibit only an *approximation* to *their* ABSOLUTE calorific values.

We have now to consider the second head into which I divided the relative value of fuels, namely, the influence of the time required by fuel to emit its heat.

The general formula of woody fibre is, carbon 36, hydrogen 22, and oxygen 22, and hence the ultimate analysis of all woods invariably indicates the hydrogen and oxygen as present in the proportion to form water without an excess of either element. It is found, however, practically, that all woods contain an excess of hydrogen, varying in quantity in different woods. Thus, in oak there is an excess of 5.08 of hydrogen, in lime of 13, in deal of 9.50, and in larch of 8.60. This excess of hydrogen, on the application of heat, gives rise to hydro-carbons, the evolution of which diminishes the carbon in the wood, and consequently decreases the residue of charcoal. The first and more rapid stage of combustion, therefore,—the combustion with flame,—is augmented at the expense of the second and more permanent, viz., the incandescence of the charcoal. The quantity of heat evolved may be the same, but the rapidity of its evolution diminishes the value of the fuel as a heating agent, for our arrangements for heating are generally of such a nature as to require time for the heat given off by the fuel to communicate itself to the part to be heated. The harder

woods, therefore, are preferable to the softer for general purposes, for although the heat evolved by them is less than that given off by the softer kinds, it is rendered more useful in the furnace by passing off with less rapidity. But the greater intensity of the heat thus rapidly produced by the softer kinds, gives them a direct advantage in some manufactures, as in that of porcelain.

The same remark applies equally to coals, and especially to those which are employed for naval purposes. It is often exceedingly important to be able to get up steam in a short time, and those coals which are admirably adapted for maintaining the fire when raised, are usually found deficient in this respect. A mixture of two coals of opposite properties is the only obvious remedy.

I will now only detain you for a short time while I make a few remarks on the improvements in the development of heat, suggested by the preceding considerations. The destructive distillation of coal, we have seen, is productive of the following compounds,—coke, tar, water, ammonia, carbonic acid, olefiant gas, hydro-carbons, and other inflammable gases. The following quantities are those resulting from analyses of six Welsh coals (one of which was anthracite), coke 85.99, tar 2.02, water 3.33, ammonia 0.18, carbonic acid 1.66, sulphuretted hydrogen 0.08, olefiant gas 0.24, other gases 5.34. Now we have seen that the heat developed during the combustion of the gaseous constituents of a coal is equal exactly to the amount of heat abstracted during their formation.

The existence of this thermo-neutrality corroborates the assertion, that the evaporative value of a bituminous coal is expressed by the evaporative value of its coke. This has been tested at great length, and it is found that the work capable of being performed by the coke alone, is theoretically much greater than the practical effect of the

original coal. From this the most useful practical results accrue. The volatile constituents might be separated from the coke, and the latter applied with equal economic effect in our manufactures, without the emission of that smoke which at present is so fatal to the health and comfort of our towns. The volatile combustible substance might, by an enlarged system of gas manufacture, be extensively employed for the development of domestic heat, as well as for illuminating purposes.

The ammonia, which we have seen to be an important product of the distillatory process, is at present discharged into the atmosphere. By the most simple arrangement, however, in the coke ovens, the ammoniacal vapours might be economised, and under the form of sulphate of ammonia form a most valuable commercial article. An ingenious patent has recently been taken out by Mr. Crole, of the Tottenham Gas Works, by which he passes the gas through a weak solution of sulphuric acid; the ammonia in the gas unites with the acid, forming sulphate of ammonia, and the gas, after the separation, is increased five per cent. in illuminating power. The solution of sulphate of ammonia is then evaporated down to dryness, every gallon of the liquor yielding 80 oz. of the salt. It has been estimated that 100 tons of coal, in coking, is capable of producing on the average six tons of sulphate of ammonia, NH_3, $SO_3 + HO$, and the commercial price of that salt for the purposes of a manure is £13 a ton. A still more beneficial process is, perhaps, that of purifying the gas by phosphate of lime, containing some sulphuric acid, as obtained by the action of oil of vitriol upon bone earth. The product,—phosphate of lime and ammonia,—containing some sulphate of ammonia, forms a still more valuable manure than sulphate of ammonia alone.

The application of coal gas for the production of heat

as well as light, is one which demands our strictest attention. The heating value of gas is even greater than that of coal, and its application to domestic purposes easy and economical. The separation from it of ammonia by the processes just now alluded to, the removal of naphtha vapour and bisulphide of carbon, which are the chief causes of the unpleasant odour of gas, and the application of all these products to economic purposes, render the gas free from all the objections to which, if used in large quantities, it would otherwise be liable. Its advantages, too, are manifest; the absence of all smoke, and, if properly ventilated, of all deposit of soot, the cheerful brilliancy of its flame, the facility with which the supply can be regulated, and the temperature consequently decreased or elevated, all combine to render the use of gas as a substitute for coal in domestic, as well as in manufacturing establishments, at once practicable and beneficial.

Mr. Appold, the talented inventor of the centrifugal pump, at his residence, near London, has erected a small gas manufactory in one of his outbuildings, and uses its products for every heating purpose throughout his entire premises; and by introducing the gas through a number of small orifices at the back of the grates, he obtains a cheerful and exceedingly warm fire. I may mention that, among other ingenious experiments he has provided each supply pipe with a throttle valve, so delicately suspended as to be opened or closed by the fall or rise of a thermometer placed on the stairs leading to the drawing room; and so admirably can the temperature of the house thus be regulated, that he has assured a friend of mine that it does not vary more than 3° or 4° during the whole year. The opening of a window in one of the bedrooms in a few minutes produces a fall in the central thermometer, which opening the supply valve and admitting a larger quantity of gas, causes an imme-

diate development of a proportionably increased amount of heat. I mention these facts to prove that the practical application of gas as a heating power is not ideal, but absolutely in operation. Nor, I believe, is Mr. Appold's a solitary instance.

One more suggestion, and I will bring this somewhat too lengthy paper to its termination. In the manufacture of iron and other metallurgical and manufacturing operations, the elimination of very large quantities of combustible gases is common. The emission of olefiant gas, carburetted hydrogen, and other hydro-carbons, is daily attested by the immense volume of flames which illuminate the country near large iron manufactories. These gases might by various expedients be collected and applied to heating and illuminating the various buildings connected with the works. In an admirable report published by Chevalier Bunsen and Dr. Lyon Playfair, on the gases existing in iron furnaces, it is clearly demonstrated that one of the largest resources of the country is wasted by the neglect of these important considerations. I do not hesitate to say, that the expenditure of fuel to produce steam power for the blast and other mechanical operations, might be wholly economised by the application of such combustible gases in lieu of it.

In all manufacturing processes we are too apt to attend solely to the production of one commercial article, and to neglect others which necessarily arise during its formation. But in these days, when a vast struggle for superiority in commerce and in agriculture, in manufacture and in science, is shaking all European nations to their foundations, the importance of any, even the smallest, bye-product, cannot be overrated; and it should be the object of the manufacturer to discover the whole of the compounds eliminated, and by applying each to its respective purpose, equalise the expense of the generation of all. I might enlarge on the

necessity of every manufacturer possessing such a general education in all sciences and all arts, as to enable him to observe what the uneducated, and consequently prejudiced, man would allow to pass by him every day unnoticed; but I will conclude, hoping, at some future time, to follow up the suggestions I have ventured to make, by a more detailed inquiry into the processes of illumination.

W Baird
5.50.64

REPORTS OF THE PROCEEDINGS

OF THE

GEOLOGICAL & POLYTECHNIC

SOCIETY

OF THE

WEST RIDING OF YORKSHIRE,

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1851.

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1852.

PROCEEDINGS

OF THE

GEOLOGICAL & POLYTECHNIC SOCIETY

Of the West-Riding of Yorkshire,

AT THE FORTY-SECOND MEETING, HELD IN THE CUTLERS'-HALL, SHEFFIELD, ON THURSDAY, JUNE 5TH, 1851.

Alderman THOMAS DUNN having taken the Chair, the following gentlemen were elected Members of the Society:—

W. H. JEFFCOCK, Esq., Sheffield.
H. C. SORBY, Esq., Sheffield.
THOMAS WILSON, Jun., Esq., Sheffield.
JOHN JEBSON, Esq., Huddersfield.

The CHAIRMAN, after addressing the Society in a short speech, called upon Mr. SORBY to read the first paper:—

ON THE MICROSCOPICAL STRUCTURE OF THE CALCAREOUS GRIT OF THE YORKSHIRE COAST. BY HENRY CLIFTON SORBY, ESQ., F.G.S.

The calcareous grit is a sandstone containing a variable quantity of carbonate of lime, which lies just below the coralline oolite. Good sections of it may be seen at Filey, Gristhorpe Bay, and Scarborough. A great part of it is only a sandstone containing a variable quantity of calcareous matter, but a considerable portion contains a great deal, and is much hardened by the infiltration of agate, which has silicified many of the shells and the wood, and filled the chambers of many of the ammonites. It is to this part of the bed that I wish to call attention.

If a piece of it be dissolved in hydrochloric acid, we obtain portions of agatized shells, and a quantity of sandy matter,

which, without further examination, would naturally be thought to be merely sand, and such it has hitherto been considered. When, however, examined with a microscope, it is at once seen to contain a very large quantity of reniform bodies, which are evidently not sand, but some kind of minute organisms converted into agate. The mere occurrence of minute agatized shells in this deposit would certainly not be a fact worthy of being laid before this Society, but since they exist in such vast numbers as to constitute a very considerable portion of a well-known rock, and, by the manner in which they occur, are presented to us in a form that is additionally interesting, perhaps a detailed account of them will not be thought a subject unworthy of attention.

The chemical composition of a specimen containing no large shells was determined by dissolving in it hydrochloric acid; and the fine muddy matter was separated from the coarser sandy portion by suspension in water and decantation; from which I found that we had in 100 parts—

Matter soluble in hydrochloric acid, being chiefly calcareous	60
Coarse sandy matter	34
Fine sandy matter	6
	100

This coarse matter consisted, in nearly equal proportions, of the reniform agatized bodies and sand, whose particles varied in size from $\frac{1}{100}$th to $\frac{1}{2000}$th of an inch, the average being $\frac{1}{200}$th, of which the great bulk was composed. The fine matter contained some few grains of $\frac{1}{700}$th to $\frac{1}{1000}$th of an inch, and all sizes down to $\frac{1}{100,000}$th or less, but the bulk was made up of those of from $\frac{1}{10,000}$th to $\frac{1}{20,000}$th of an inch. Hence it is clear that this deposit was not subjected to much washing at the time of its deposition, or else it would not have been composed of particles of such very varying magnitude. Although in some parts the coarse

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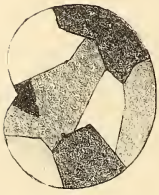

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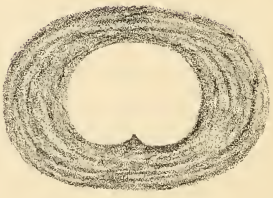


H. C. SORBY. DEL ET LITH.

sand and reniform bodies occur in such numbers as to touch one another, and although some portions of the grit retain their figure when the calcareous matter is dissolved away, yet in others it may be seen that a considerable part of it was deposited at the same time, and was not merely infiltrated among the coarser particles subsequently, although much of it has undoubtedly been introduced in this manner.

If a thin slice of the stone be prepared, not much thicker than $\frac{1}{1000}$th of an inch, and examined with a microscope, the manner in which the grains of sand and the reniform bodies are imbedded in the impure calcareous deposit is well seen. The form of these bodies, however, is best made out by examining them as left after dissolving a portion of the stone in acid, when it will be seen that they vary somewhat in their shape. In many cases they are of a form that may most truly be termed reniform,—one side being perfectly rounded, and the other having an incurved depression of varying depth; in some so slight that the body is almost globular, as shown by *Fig.* 1, *Plate IV.*, which, as well as all the rest, except *Fig.* 8, is magnified about 200 times linear. When turned round, they are seen to be more or less depressed, and to have an oval form, both sides being symmetrical, as shown by *Fig.* 2. Their size varies from $\frac{1}{1500}$th to $\frac{1}{4000}$th of an inch, the average being about $\frac{1}{2000}$th. Their internal structure is best seen by mounting them in Canada balsam, which, having nearly the same refractive power as the agate, causes the light to pass through them as regularly as if we had a thin section. I have also carefully investigated it by examining them in the latter condition, as occurring in a thin slice of the stone. By these means I find that they often have a structure similar to that frequently seen in large agates, the deposit having begun from the sides, and left a vacancy in the centre, which was afterwards filled up with less pure agate in alternating, more

or less coloured strata, as shown by *Fig.* 3. It is only in some cases that this sort of structure is well seen by transmitted light; but when a thin slice is examined by reflected light, it is seen much more frequently, owing to the circumstance that many, in which it is really present, are occupied with colourless agate, and, under the former condition, do not, therefore, exhibit their component layers. In some few rare instances there is a vacant space left in the centre, owing to this last stage of agatization not having occurred; and often the whole is filled with homogeneous agate. There are also cases where there is more or less of granular impurity in them, either disseminated or collected together.

But not only are these reniform bodies converted into agate, but also very frequently are entirely filled with calcareous spar. The difference between agate and calcareous spar is very readily discoverable by their action on polarized light. Calcareous spar, owing to its very intense double refraction, does not give rise to colours in the same manner as quartz or agate, in which the double-refracting power is so much less; but, unless the slice be extremely thin, except when we see portions of its system of rings, the only action which it has on polarized light, is to depolarise it in some positions as white light, and, by rotation, definite neutral axes are found. Thus, if the whole of the reniform body is filled with calcareous spar, having the crystalline particles all arranged in one direction, by rotating it round the beam of polarized light, with the analyser in such a position as entirely to suppress it, in some positions white light will be seen to pass through the whole, while in others the body will appear entirely dark. In other cases the bodies are filled with calcareous spar, which has been crystallized in portions that have their neutral axes inclined to one another at various angles, which gives rise to an appearance similar to that seen in *Fig.* 4; the dark or

lighter divisions, of course, changing in intensity by rotation, but retaining their forms, which are angular, and such as would be produced by the infiltration of calcareous spar into a shell. When, however, they are filled with agate, no such effect is produced, but in every position we have the appearance of a white disk, with black bands radiating in an irregular manner from the centre, (as shown by *Fig.* 5,) and changing their forms and places by rotation, whilst the intermediate parts have patches of orange or blue, which properties agree with what would result from such an irregular structure related to the centre as the infiltration of agate would most probably produce. In some cases, one portion is converted into agate, whilst another is calcareous, sometimes in the form of crystalline spar, and at others as a concretion, similar to those sometimes to be seen in the substance of the shells from the grit, which are partially agatized. In these, some of the original calcareous matter still remains in the form of small concretions, sometimes disseminated in the agate, but generally attached to the sides of the shell. In the same slice, and in the same field of view of the microscope, we find some of the reniform bodies filled with agate, and others with calcareous spar; but their relative numbers vary in different parts, in some parts nearly all being agate, and in others calcareous.

I counted in the space of $\frac{1}{280}$th of a square inch, in an average portion of a thin slice, no less than forty of these bodies, and, therefore, on a square inch there would be 11,200; and since they are, on an average, $\frac{1}{200}$th of an inch in diameter, there would be at this rate about two and a quarter millions in a cubic inch. Moreover, taking into consideration those filled with calcareous spar as well as those that are agatized, I shall not be overrating them if I suppose them to constitute 20 per cent. of the whole rock. Calculating on this supposition, and on their being $\frac{1}{200}$th

of an inch in diameter, and, therefore about $\frac{1}{15,000,000}$th of a cubic inch in content, there would be about three millions in a cubic inch. Hence I think it may be safely considered that there are two or three millions of them in a cubic inch of the stone, when it does not contain many larger shells.

Having thus described these reniform bodies, and shown in what vast numbers they are found. I will consider what they are. That they cannot be grains of sand is quite certain, when we compare their forms and optical properties. Grains of sand are usually considerably angular, and, when more rounded than is common in the rock under consideration, have a form similar to that shown by *Fig.* 7. When seen by reflected light, if mounted in Canada balsam, they are clear and transparent, and not milk white, like the reniform bodies, and never show anything like the agate structure seen in *Fig.* 3. Using transmitted polarized light, they show a definite single system of neutral axes, giving in some positions the even dark appearance shown by *Fig.* 7, and in others various simple colours of a low order, and never any radiating bands at all like those presented by the reniform bodies, (as shown by *Fig.* 5.)

Those which are calcareous do not appear to be rounded grains of shell or coral, for they are, similarly, much too regular in their form. I have had it suggested to me that they might be calcareous concretions similar to the ovum-like ones of oolite rocks, those which are now agate having subsequently been converted into it. I, however, think that when the characters of oolitic grains are compared with them, there is very good evidence that they are not. In all the oolitic rocks which I have examined, the concretions are composed of alternating concentric layers of more or less impure calcareous matter, formed round grains of sand, fragments of shells or coral, or entire small shells, (as shown by *Fig.* 8.) On the contrary, that with which some of the

reniform bodies in the calcareous grit are filled is usually nearly pure, whilst that in the intermediate spaces contains much impurity. In those few cases where that in the reniform bodies contains a little impurity, it does not show any trace of concentric structure, but is in precisely the same state as that in the chambers of the ammonites which occur in the rock. Moreover, oolitic concretions are often very irrregular in their forms, and frequently two or more coalesce, which is the reverse of what is found to occur with the reniform bodies under consideration. I have found, by examining the oolitic portions of the coralline oolite, that they also exist in it, though in no great numbers. In it the difference between them and the concretions is very marked, and they form the nuclei of some of them (as shown by *Fig.* 8, which is one magnified 150 linear,) precisely in the same manner as several species of well-known forms of foraminifera and other small shells. At first I thought that they might be globular siliceous spiculæ of sponges, but after finding some which were filled with calcareous spar, I abandoned the idea, for it does not appear to me at all probable that the siliceous matter could have been removed, and its place occupied by calcareous spar.

I have already stated that these reniform bodies occur both filled with calcareous spar and with agate. This is precisely the same with the ammonites which are found in the rock along with them, and I, therefore, judged that it was probable that they were shells similarly filled with infiltered mineral matter. To ascertain this, it was, of course, necessary to determine whether they originally possessed shells or no. I therefore carefully examined a section of a portion of the rock containing an ammonite filled with calcareous spar, in order to learn what kind of evidence it presented of the former existence of a shell. By examining it by polarised light, I found that where no impurity had entered, there was no trace whatever of the shell

discoverable, the whole of it having its crystalline particles arranged in the same direction as the calcareous spar in the interior. Where, however, there was muddy impurity in the interior, the shell could readily be distinguished. Moreover, by examining the ammonites which are agatized, I found that the exterior shell was very frequently distinguishable from the infiltered agate, by being more transparent; but this transparent part was not so thick as the shell originally was, and in some cases did not occur at all. The septa, however, being much thinner than the exterior shell, could not be detected in the agate. These facts, therefore, show that, though we cannot detect any trace of shell in the reniform bodies which are filled with pure agate or calcareous spar, we are not warranted to conclude that it did not at one time exist. On this account I very carefully examined a thin section of considerable size, in order to ascertain whether there existed indications of any of these bodies having been burst in, and whether the arrangement of the impurities introduced by that means afforded evidence of the former existence of the shell.

Although such cases are rare, yet I have found some which unquestionably proved that they had been broken, and from the manner in which the impurities occur, I think that the existence of a shell is rendered very probable. In one instance the section of an agatized reniform body showed that it had been ruptured at one end, and the impurities introduced by that means into the interior are collected in such a manner as clearly to indicate the existence of a shell; a distinct uniform line or space of clear agate occurring between the outer boundary of the reniform body and the extraneous granular matter that partially occupies its interior. In another instance, a section of a calcareous body exhibits a thin layer of fine granular matter following the curvature of the inner surface of the reniform body, and lying within, and at a slight uniform distance from its outer

boundary, whilst a straight elongated fragment lies within, and rests its ends against the line of granular matter (as shown by *Fig.* 6). Hence it appears that both the finer matter and the larger extraneous fragment rested originally on the inner surface of a shell that has become crystalline. The thickness of the shell shown by both these specimens was about $\frac{1}{7000}$th of an inch, which is so thin that we could not expect to find any trace of it in the agate, when the septa of the ammonites are obliterated. These facts, I think, indicate that these bodies were small shells, whose interiors have been filled with calcareous or siliceous infiltrations, in the same manner as the chambers of the ammonites which are found in the rock. Nevertheless, I will not insist on this view, for I have found cases where the impurities were not arranged as if there had been a shell, though not in a manner irreconcilable with that supposition; but, on the whole, I should say, that there is better reason for thinking them to have been shells, than any other bodies with which I am acquainted. If really shells, they may perhaps have been *Foraminifera*, although I have not been able to detect any internal divisions into chambers, nor anything to indicate that they are detached foraminiferous cells, such as are sometimes met with.

Besides the reniform bodies, we find in the matter left undissolved by acid several other minute agatized organic bodies, which it may, perhaps, be as well to describe. There occur minute cellular bodies (zoophytes?) formed of more or less oval open cells, concave on one side and convex on the other, placed end to end, and communicating at the extremities, in a similar manner to what is seen in the section of a *Nodosaria*. The cells are frequently irregular, and vary much in form; two or more rows being often placed side by side, and single rows of cells frequently branch off from the main body.

Numerous spiculæ of sponges are also found, the most common of which are simple, smooth, and pointed at each end. Others are bulbed at one end and pointed at the other; sometimes of considerable length, but usually broken. I have also found one much branched, quadrifid, with two or more of the extremities bifid.

In some sections I have observed fragments of a brown colour, apparently vegetable, and having a similar structure to portions of *Algæ*.

ON THE USE OF THE BAROMETER IN COAL MINES, AND A CHEAP FORM OF INSTRUMENT FOR THAT PURPOSE. BY W. SYKES WARD, ESQ., F.C.S., OF LEEDS.

Mr. Ward commenced by observing that he should not on that occasion attempt to discuss the necessity of using the barometer to prognosticate the state of the air in coal mines, as that had been proved in a paper by Mr. West. His object was to introduce the barometer to prevent accidents in mines. It is not the absolute height at which the barometer stands which will indicate danger, but the amount of diminution of pressure or rarefication of air which has taken place within a given time. Suppose the waste or goaf of a mine contain ten times the cubic capacity of the parts of the mine regularly worked and ventilated, if the barometer fall even as low as 28 inches, so that it fall very gradually, the ventilation may carry away the carburetted hydrogen issuing from the goaf and fissures, and no danger be perceived. But if the barometer suddenly fall one-tenth of an inch, the gas in the goaf would expand one-hundredth part, throwing into the workings one-tenth of its volume, and producing an explosive mixture. To guard against that there should be a regular series of barometric observations taken in each

pit, and the record sent to some party who might compare the observations with others taken either in other pits, or by parties keeping a regular register, at a considerable distance. Such comparison of registers would produce an efficient check on the man who had to record the barometric variations. At least four observations should be taken during the twenty-four hours, and whenever any fresh set of men went down to work, so that they might receive timely caution of danger.

The method of recording the variation should be as simple as possible. A slate or piece of board should be furnished, ruled with columns and spaces for each time of observation, and on which the height of the barometer might be marked with chalk, and, for simplicity, it might be advantageous to use Roman numerals, and to make O represent 28 inches, X 29 inches, XX 30 inches, and a simple stroke I for each tenth up to half an inch, then the V, and for one-twentieth a short stroke, thus ı; and on a third line mark the number of tenths by single strokes by which the barometer, at any observation, is lower than the preceding one; thus making the amount of fall appear evident without necessity for much calculation. Thus, if at six o'clock in the morning the reading were 30.25, at noon 30.40, at six o'clock in the evening 29.90, and at midnight 29.40, the table would be as follows:—

6	XX.IIı	
Noon.	XX.IIII	
6	X.VIIII	IIIII
12	X.IIII	IIIII

After exhibiting this method, by which a wholly illiterate man might take and record the barometrical observations, Mr. Ward described a form of instrument that he had contrived, which consisted of a simple tube of small bore, and of thin glass, which he had found rendered the boiling of mercury a comparatively easy operation, and to which need only be added a small bottle of mercury by way of a cistern, and a scale divided into twentieths of an inch, and which would be better done by comparison with a standard instrument than by actual measurement. The whole might then be protected by being inclosed in an iron tube, such as is used for gas. Such an instrument would be sufficient, and might, with a moderate demand, be probably supplied at a cheap rate, say from 10s. to 15s.

Mr. T. D. Jeffcock inquired in what part of the pit it was proposed to place the barometer?

Mr. Ward said that perhaps the best situation would be the place where the candles or lamps were kept, so that it might be seen by the men, and, if necessary, they might receive a caution before they began to work. He wished it to be understood that he was speaking of mines in which exposed lights might, under ordinary circumstances, be used. Where safety lamps were required, the barometer was of no use: every precaution was indispensable, and there might be danger, though not indicated by the barometer.

In reply to another question by Mr. Jeffcock,

Mr. Ward said it would not be necessary to have a barometer in every part of the pit where the men worked, as its object was not to indicate the amount of inflammable gas; it was simply designed to show when the atmosphere was low, that being the time when most gas escaped from the goafs and fissures.

The Chairman remarked that nearly all the explosions in the north had happened in a low state of the barometer

after a sudden fall. In this neighbourhood, where a totally different system of working prevailed, explosions had mostly proceeded from an insufficient circulation of air in the goafs or exhausted parts, to dilute the gas as it escaped from the coal, and reduce it below the inflammable point. Of the usefulness of the barometer there could scarcely be two opinions. It was a singular fact, that the illiterate miners know instinctively that a south wind induces bad air in the pits. In this neighbourhood they imagine that the south wind blows through the whole strata, which inclines upwards to the south, and that the mine is thus filled with bad air. But however false in philosophy, being an observation founded on fact, it is practically of use to the miner, because, as a general rule, the south wind is accompanied by a low state of the barometer, and a sluggish circulation of air as a consequence. The barometer, by showing the pressure of the atmosphere, indicates the amount of escape of gas, and induces caution, on account of which it is a very necessary instrument. It was not necessary that the barometer should be kept in the pit at all. The engineman at the top is generally the best educated, and if under his charge any portentous change should occur, he could communicate instantaneously to all parts of the pit.

Mr. Ward.—I do not see the least objection to that.

Some further discussion was continued between Mr. Ward, Mr. Sorby, and Mr. James Haywood, analytical chemist.

The Chairman here called for the last communication announced for the meeting, which was

ON SOME PHENOMENA AND EXPERIMENTS UPON CAPILLARY ATTRACTION. BY MR. T. J. PEARSALL, F.C.S., OF LEEDS.

The leading object of Mr. Pearsall appeared to be to suggest the constant interference of mechanical forces of

solids, with chemical qualities of the atoms, as necessary to explain many of the conditions of articles used for comfort and convenience; on which, indeed, their usefulness for the purposes of buildings, clothing, and even for food depended.

Calling especial attention to the action of the surfaces of all solids as exercising mechanical forces, and also rapidly allowing the peculiar influences of heat, light, and electricity, to be absorbed or reflected, and the action also of the surfaces of fluids upon air and gases, he then called attention to the attractions of a surface of liquid, for the surface of a solid body: considering this a force of great and overlooked importance in nature, because called capillary attraction, and usually understood only to relate to exceedingly small surfaces, and hair-like columns of fluids, as the examples of power.

He proceeded to shew that a very large class of unexplained phenomena was referrible to the agency of capillary attraction, a fuller acquaintance with which might lead to the surmounting of many scientific and practical difficulties. The process of carbonizing steel—the penetration of the carbon into all parts of a substance not supposed to be porous—was incidentally referred to, as showing how little is known of some highly important processes beyond the mere results. Mr. Pearsall undertook to show that the force termed capillary attraction, was in that special form no more than a practical definition of a much more general form, exercised not only upon small quantities of matter, but upon a large scale. To exemplify the operation of capillary attraction, recourse was had to a conical glass vessel, terminating in a tube having a bore equal to a human hair. When the vessel was inverted and immersed in water, the liquid, in defiance of the laws of gravitation, rose up into the tube (which was not immersed) to a height of several inches above the water. This was ascribed to the

attraction presented to the water by the wetted sides of the interior of the tube. A simpler illustration was also referred to—the placing two flat pieces of glass against each other, and resting the lower edge in water, whereupon a watery film is drawn up between the two surfaces. Having shown the principle of capillary attraction in the well-known case of a single tube, Mr. Pearsall now exhibited it on a large scale. A hemisphere of fine wire gauze was made to float on water, by simply establishing a watery film in all the interstices by means of immersion, the resistance of the film to the inclosed atmosphere being so great as to keep the copper hemisphere afloat, and sustain a pendant weight of one ounce. The converse of this experiment was shewn by filling a wire gauze sphere with water, and keeping it in by the mere strength of a similar film procured by immersion.

It was thus proved that a thin plate of liquid, even water, (and that might be made up of many small plates), possessed a force that could retain imprisoned volumes of air and gases that could not escape, even if assisted by the pressure of liquids in bulk, and that the thin film of liquids could also retain cubic inches of fluids suspended, and on all sides surrounded by the atmosphere. After shewing the importance of this to explain the retention of air or liquids by membranes, and so causing diseases, and of the retaining power exercised by the structure of strata to retain liquid and gases, he shewed an application of this power on the large scale, and, as some might call it, of practical application and suggestion. The experimenter took an ordinary pillow-case, well wetted, and inflated it with atmospheric air, when it was observed that all the interstices of the threads being occupied by water in the form of film, the object, thus rendered to a surprising extent air-tight, became such a resistant buoy as to be capable of floating a person, or several persons, on the water. This expedient, it was suggested,

might be resorted to as a temporary float in a moment of emergency; or drifting upon the ocean, to convey a line from a distressed ship to the shore; the only condition requisite to sustain the floating qualities of the buoy being the continued wetness of the surface, which would retain the atmospheric air. Capillary attraction was exhibited in the absorption of moisture by inferior bricks of houses, and by various articles of clothing. This interesting subject was so treated as to render it popular and highly attractive; and it is much to be regretted that there was not a larger company to profit by the results of Mr. Pearsall's philosophical research. A promise was given for a renewal of the subject on a future occasion.

PROCEEDINGS

OF THE

GEOLOGICAL & POLYTECHNIC SOCIETY

Of the West-Riding of Yorkshire,

AT THE ANNUAL MEETING, HELD IN THE EXCHANGE-ROOMS, BRADFORD, ON FRIDAY, OCTOBER 17TH, 1851.

In consequence of the indisposition of the Right Hon. Earl Fitzwilliam, F.R.S., President of the Society, Alderman DUNN, of Sheffield, was called to the Chair.

After the election of the Officers and Council for the succeeding year, the annexed statement of the accounts for the past year was submitted by the Treasurer:—

STATEMENT OF THE RECEIPTS AND EXPENDITURE,

From 29th October, 1850, to 17th October, 1851.

DR.	£	s.	d.
To Balance in Treasurer's hands..	9	14	7½
To SUBSCRIPTIONS from—			
1 Member for 1839 at 10s. 6d...	0	10	6
1 „ 1840 „ ..	0	10	6
1 „ 1841 at 13s.	0	13	0
1 „ 1842 „	0	13	0
1 „ 1843 „	0	13	0
1 „ 1844 „	0	13	0
1 „ 1845 „	0	13	0
8 Members for 1846 „	5	4	0
1 Member for 1846 at 10s. 6d...	0	10	6
9 Members for 1847 at 13s.	5	17	0
1 Member for 1847 at 10s. 6d...	0	10	6
10 Members for 1848 at 13s.	6	10	0
1 Member for 1848 at 10s. 6d...	0	10	6
22 Members for 1849 at 13s.	14	6	0
1 Member for 1849 at 10s. 6d...	0	10	6
42 Members for 1850 at 13s.	27	6	0
67 „ 1851 „	43	11	0
1 Member for 1851 at 10s. 6d...	0	10	6
3 Reports at 2s. 6d. each	0	7	6
	£119	14	7½

CR.	£.	s.	d.
By Printing Reports, &c.	10	0	0
„ Advertising Meetings.....	2	14	0
„ Lithographing Plates.....	2	0	6
„ Expenses of Meetings	3	17	5
„ Collecting Subscriptions	1	9	0
„ Postage Stamps and Postages..	1	5	9
„ Books and Stationery	1	2	2
„ Rent of Museum Room	20	0	0
„ Salary of Assistant Secretary..	50	0	0
„ Map of Yorkshire Coal Field..	1	5	0
„ Sundry Expenses	3	4	7
„ Balance in the hands of Mr. Batman	10	18	6
„ Balance in Treasurer's hands..	11	17	8½
	£119	14	7½

The Chairman then called for the first communication, which was a

DESCRIPTION OF A DIRECT-ACTING STEAM FAN FOR THE BETTER VENTILATION OF COAL MINES. BY JAMES NASMYTH, ESQ., OF PATRICROFT, MANCHESTER.

Mr. Nasmyth began by referring to the disastrous accidents which had arisen from defective ventilation,—within the last two years there having been 190 lives lost in the neighbourhood of Barnsley alone. This wholesale destruction of human life urgently called for an energetic effort to remove the evil. By increased means of ventilation, and thus getting rid of the accumulation of explosive gases, they would be striking at the root of the evil. This had been hitherto done by causing a rarefaction of air in one of the ascending shafts of the mine by means of a furnace burning below. It had, however, been proved that the furnace system of ventilation was deficient, disastrous accidents having occurred in mines so ventilated. Some more efficient means of sweeping away the accumulation of impure air was therefore called for, and Mr. Nasmyth recommended his direct-acting fan as supplying the *desideratum*. Economy was among its advantages: if one-tenth of the coal at present burnt in a furnace to produce direct rarefaction, were burnt under a boiler applied to a small engine for the purpose of working the fan, it would generally produce a much greater effect. Another important advantage possessed by this ventilating apparatus was, that it was above-ground and accessible, instead of being below-ground, as at present. In case of accident they would thus have full means of re-ventilating the mine. At present many hours had generally to elapse, in case of accident, before the mine could be sufficiently re-ventilated to render a descent safe, and probably one-half of the sufferers died a lingering death from suffocation, and not from the immediate effects of the ex-

plosion. The steam fan was about to be put in operation near Wakefield, and its simplicity of action recommended it to all who had seen it. Mr. Nasmyth explained the action of the fan by referring to a drawing which he had prepared. On the fan being made to revolve rapidly, the volumes of air contained between the vanes were forced out, while fresh air rushed in to supply its place. He recommended that the steam engine should be placed in direct communication with the fan, and that the engine should have a very short stroke. A speed of 300 revolutions per minute was thus very easily attained, and 20,000 cubic feet of fresh air per minute could be forced into a pit with a fan whose vanes were eight feet in diameter. A fan whose vanes were double the diameter would be amply sufficient to ventilate the largest mine in Britain. The whole apparatus, including steam engine, would cost about £160. With so efficient a means of ventilation, attainable at so small a cost, it would be a disgrace, in these times of civilization, if such wholesale destruction of human life were allowed to occur.

The second communication, being upon an apparatus for effecting the same important object, was read by Mr. Biram.

DESCRIPTION OF THE FAN ERECTED FOR THE VENTILATION OF THE HEMINGFIELD PIT, BELONGING TO EARL FITZWILLIAM, AT ELSECAR. BY B. BIRAM, ESQ.

This fan, which is eight feet diameter and nine inches wide, has eight vanes, which are so arranged that each forms the segment of a screw which revolves in the same plane. The angle of the extremity or outer edge of the vanes is about 23 degrees with the plane of rotation, making the pitch of the screw about 10 feet 10 inches. The area for the passage of air through the fan is about 47 superficial feet, which, multiplied by 10 feet 10 inches, —the pitch of the screw,—gives 509 cubic feet as the

theoretical quantity of air which would pass through the fan each revolution. The fan makes 110 revolutions per minute, and is worked by the pressure of a column of water 450 feet high, acting upon the piston of a small hydraulic engine, working in a cylinder four inches diameter and one foot stroke. The engine makes 20 double strokes, consuming 21½ gallons of water per minute; so that the power expended to overcome the friction of the machinery and propel the air is equivalent to the weight of the above quantity of water, or 215 lbs. falling through 450 feet per minute, equal to three horses' power nearly. The engine is considered not sufficiently powerful, and it is intended to increase the power; but the passage of the air is also obstructed by a contraction between the fan and the upcast shaft, which will be remedied by a slant drift, nearly completed, for conveying the air direct from the fan into the upcast shaft. At present the circulation is increased by a fall of water issuing from an opening of five square inches area, and a head of three feet six inches from the top of the engine or downcast pit, which it is calculated discharges 120 gallons per minute. The combined effect of the waterfall and fan causes a circulation of 27,700 cubic feet of air per minute. When the fan is not at work, the waterfall alone circulates 12,700 feet per minute, which leaves 15,000 cubic feet due to the effect of the fan. A comparison of the performance of the water employed in each case, and the power necessary to raise it again to the height from which it falls, will be as follows :—

	Gallons.		Feet.	Horse power.
The fan uses per minute.....	21½	falling	450	... 3
The waterfall ditto	120	„	450	... 16

Mr. Biram considered that this description of fan passed as large a quantity of air as any that he had seen. It might be fixed in any situation, and connected with any motive power, whether water or steam. Mr. Biram exhibited a beautiful working model of his fan, about a foot in diameter, the

experiments with which were perfectly successful. The quantity of air which passed through the fan was (approximately) indicated by an anemometer, and the number of cubic feet per minute was found to be very large.

In the course of the discussion which followed the communications of Mr. Nasmyth and Mr. Biram, the Chairman took occasion to express his opinion that the importance of this mode of working mines could not be too highly appreciated. There was one difficulty, however, connected with this mode of ventilation, which was, to regulate the velocity of the air which was made to pass through the mine so as to make it compatible with the working of the seam of coal. In case of accident in a mine, and the ventilation becoming suspended, the fan was calculated to be of great advantage, as considerable time must elapse before the furnaces could be lighted. He had been pleased with both the papers. They referred to a subject intimately connected with the commercial interest and prosperity of England. Our very existence as a commercial nation depended upon our mineral wealth, and every effort made to diminish the risks run by the miner ought to be received with gratitude and respect.

Mr. Nasmyth pointed out another advantage likely to accrue from this mode of ventilation, in diminishing the high temperature which was known to prevail at great depths; thus promoting the health and comfort of the miner and the productiveness of the mine.

Mr. Biram suggested the adoption of a lamp for use in mines, having an opaque back, with a reflector, for the purpose of throwing the light to the point where it was required.

Mr. Theodore West was glad to find the opinion gaining ground that the safety lamp was imperfect, and thought it desirable that this should be increasingly impressed on the miner, that every other precaution might

be taken. With respect to the ventilating fan, at the high speed at which it was intended to work, he thought some slight gearing might be necessary.

Mr. Nasmyth now proceeded with his last communication, which, like its predecessor, was illustrated by diagrams.

DESCRIPTION OF AN IMPROVED SAFETY VALVE FOR STEAM BOILERS. BY JAMES NASMYTH, ESQ.

Mr. Nasmyth gave a description of an improved safety valve for steam boilers, and referred to the calamitous boiler explosions which had been so frequent of late, and observed that, as a means of prevention, none was more efficient than a good safety valve. He proceeded to inquire what were the causes of the failure of the ordinary safety valve—wherein was it deficient? The function of a safety valve was to have some part of the boiler which would open and allow the steam to escape on a certain pressure being reached. This object was very simply attained by a valve of the ordinary construction, loaded either by a weight attached directly to the valve, or by a small weight increased in its efficiency by a system of levers. The ordinary safety valve (of which a model was shown) was liable to stick fast, owing to the muddy sediment of the water collecting round the spindle of the valve. In the common construction of valves the conical form was generally adopted. It was thus important that the internal and external cones should exactly coincide, and it was necessary to guide the spindle very correctly into the centre. This arrangement was subject to the inconvenience of the spindle sticking fast on the one hand, or if it were too slack, it would fail to perform its functions. It was highly probable that many accidents had arisen from the safety valve not having acted efficiently. His improved safety valve consisted in doing away with the spindle altogether, and employing the

sphere. It was the property of a sphere to fit a circle in all directions. In order to secure the perfect freedom of the valve at all times, *per se*, and without the necessity for the engineman experimenting upon it, he had resorted to a very simple expedient, by contriving that the wave-like action going on in the water should communicate a slightly pendulous motion to the valve, sufficient to prevent its being fixed in its seat. He would do away with the system of leverage, and have a direct weight attached to the valve, and placed inside the boiler, so that the engineman would be unable to tamper with it. This valve possessed another recommendation—that of cheapness, at it could be made for less than the ordinary cost.

Several gentlemen spoke in high terms of approbation of Mr. Nasmyth's safety valve, considering it the most simple and efficient which had come under their notice. A case was mentioned in which the ordinary safety valve, although tried every day, acquired a considerable amount of friction, and several cases were referred to in which it had been tampered with by reckless enginemen. On board a steamer the valve had been tied down, and in another case a man was found to be hanging at the end of the lever! In the case of an accident for which the man was sent to take his trial at York, the spindle was found to be bent, but whether this occurred before or after the explosion, could not be ascertained. Hence the importance of the safety valve being out of the reach of the person having the control of the engine.

Mr. West stated that when travelling on a locomotive, if they were behind time, he found that the engineman almost invariably held on to the safety valve, knowing that very little risk was incurred, and that lost time must be made up. He mentioned another case, as showing how precautions, apparently the most effective, may be defeated

if men are so disposed. While travelling through Belgium and France, to acquaint himself with the system of railways in those countries, he was much interested in the government steam guage for preventing undue pressure on the boilers. It was self-registering, so that at the end of the journey the amount of pressure at different stages could be seen. The engine-driver, however, by abstracting or adding a small portion of the mercury, could increase the pressure without the register giving any indication of the fact. The register acted, indeed, but only when the pressure was excessive.

The CHAIRMAN was much pleased with the improved safety valve, and thought it approached as near perfection as possible.

Mr. NASMYTH stated that six of these valves had been in action about eight months at the Oxford Road Twist Company's works, Manchester, and that the managers were most willing to receive the visits of persons interested.

ON THE CONTORTED STRATIFICATION OF THE DRIFT OF THE COAST OF YORKSHIRE. BY H. C. SORBY, ESQ., F.G.S.

The contorted stratification of the drift appears to have been little noticed or understood, until prominently brought forward by Sir Charles Lyell, in a paper read before the Geological Society in 1840, and subsequently published in the Philosophical Magazine. He then suggested that those seen on the coast of Norfolk had been produced by the action of stranded icebergs; but the disturbed condition of the chalk has led many geologists to hesitate in adopting that theory. Since then the subject seems to have been much overlooked, so much so that, when the origin of the drift was under discussion at the meeting of the British Association last year at Edinburgh, no mention whatever was made of the contortions; and yet, as I hope to show, they are of such a character as to be of great value in

FIG. 1.

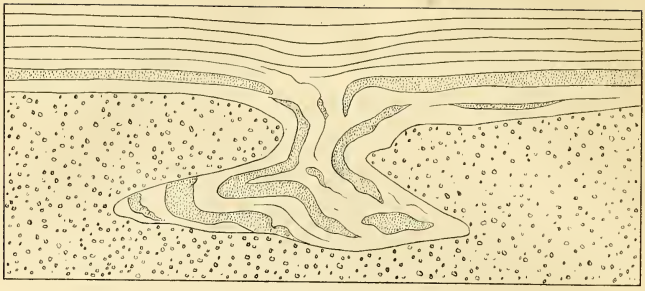


FIG. 2.

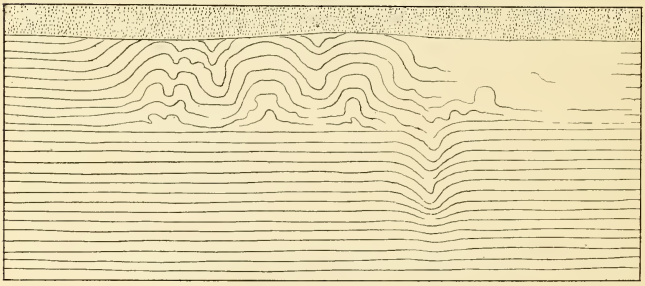


FIG. 3.

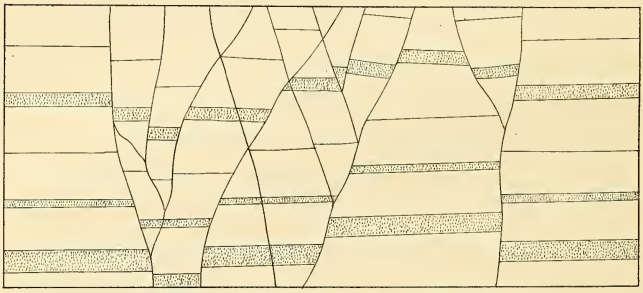
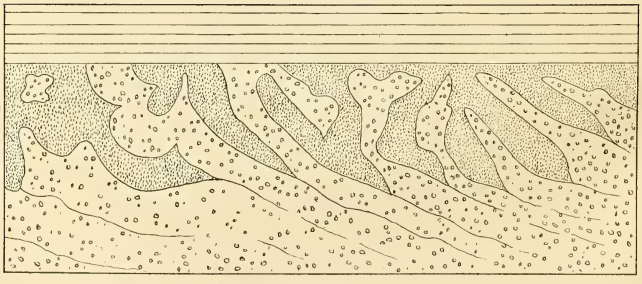


FIG. 4.



arriving at a correct theory of its accumulation. Since then a paper has been read at the Geological Society, by Mr. Trimmer, in which he attempts to explain them by supposing that masses of ice had been fixed amongst the beds when they were deposited, and that, on these subsequently thawing, the strata above sunk down into the space they occupied. Both these suppositions are undoubtedly true causes, and in some parts of the globe contorted stratification must now be produced by the action of both. My own opinion is that some of the contortions in the drift are due to one, and some to the other, and that neither alone is sufficient to account for all that occur.

Those to which I shall particularly call attention are in the neighbourhood of Bridlington; for the drift there, consisting to a great extent of horizontally stratified thin-bedded sand and clay, exhibits many facts which cannot be observed when, as is frequently the case, the boulder clay constitutes nearly the whole.

In the first place I will describe an example of what appears to me to require the explanation suggested by Mr. Trimmer. This is shown in *Fig.* 1, *Plate V.*, in which, as well as in all the others, those parts with round marks are boulder clay; those with dots, gravel; and the rest more or less fine sand: the line drawn below each being the scale of one yard. At the lower part is boulder clay, in which there appears to have been a kind of inverted funnel-shaped cavity, much broader at the bottom than at the top. This is now filled with gravel and sand, which is arranged in such a manner as to indicate that it entered through the opening at the top in a mass, and not as if deposited in it gradually. The beds just above are broken up, as though they had in part subsided into the supposed vacuity, but a little higher up they are horizontal and undisturbed. Such a case as this, I think, could be readily explained, by supposing that a mass of ice was lodged amongst the boulder clay, over

which beds of gravel and sand were deposited, which, on the subsequent thawing of the ice, sunk into its place; horizontal beds of sand and clay being afterwards accumulated over the whole.

The contortions which occur in the fine-grained, thin-bedded, sandy clay are, on the whole, perhaps the most striking. The general character of those in it, as well as in other deposits, is that at the bottom are perfectly undisturbed beds, and over these, the junction being quite sudden and distinct, are others of precisely the same nature, but bent and twisted, in the most remarkable manner, into very complicated and intricate curves. The surface of the contorted beds has then been reduced to a level, the projecting parts removed, and other undisturbed horizontal ones accumulated over them.

We are hence compelled to admit that no disturbance from beneath could have given rise to the contortions, for in that case the beds below would have partaken in the movement. Neither could any acting since the accumulation of the upper beds, or else they could not have remained horizontal. These facts, and especially the connexion of the disturbed part with those on each side which are not so, will be best seen from the very limited case shown in *Fig.* 2. At the bottom is a mass of thin-bedded fine sand, quite undisturbed; but at the upper part, where of precisely the same nature, even ripple marks being traceable, the beds are bent up into great curves. As will be seen, the contorted beds pass on the left side into perfectly undisturbed ones, whilst, on the right, they end, more or less abruptly, in a mass of fine sand, showing little or no trace of stratification, but farther on becoming regularly bedded.

Now such a case as this, I think, proves most clearly that there has been some force applied on the right hand side, which broke up the beds and pushed them along into curves towards the left; and it appears to me that an iceberg is

the only probable agent which could have done this. In this instance, the contorted portion extends only five or six yards, but it frequently is ten times that length, and then the curves are often very much more complicated. The upper part has been washed level, and a horizontal bed of gravel deposited over it; though fine-grained thin-bedded sand and clay are often found just above the contortions, and hence they cannot have been produced by any violence of current; though I have seen cases in confusedly bedded sandstone rocks which appeared to require that explanation.

But besides those contortions which seem to have resulted from the action of a lateral force, there are others which indicate one proceeding from above. In some parts, of limited extent, it is not uncommon to find the beds of gravel and sand full of small faults, such as shown in *Fig.* 3, and by tracing the beds laterally, I found that the disturbed portion was depressed below the general level. This fact, as well as the character of the faults themselves, leads to the inference that they resulted from a force acting, not from below, but from above. Another sort of case, indicating the same fact, is shown in *Fig.* 4. Here, at the bottom, is boulder clay, rudely stratified, and, as will be seen, the beds have been broken up along their partings, and gravel forced in amongst them. It is extremely improbable, not to say impossible, for these deposits to have been accumulated as they now are; but I think the supposition of the action of a force from above would explain the facts of the case. In this instance, also, it will be observed that the upper surface was reduced to a level, and horizontal undisturbed beds of fine sand and clay deposited.

I, therefore, contend that the contortions I have described indicate a lateral force, or one acting from above; and since there are so many other facts leading to the conclusion that icebergs were, to a great extent, instrumental in depositing the drift, I think that we must also refer these

irregularities of stratification to their lateral movement or downward pressure at the fall of the tide, when left stranded. The directions in which the pushing force must be supposed to have acted also agree with this theory. Though there is evidence of much oscillation in the level of the land at the period of the drift, the character of the shells found in it at Bridlington indicates a depth of not more than 100 feet, and that dry land was near. This would lead to the inference that the chalk wolds were then above the sea, and, if so, there must have been a kind of bay at Bridlington, somewhat similar to the present. I have very carefully investigated the directions of the currents in this bay at the period of the drift, as shown by the ripple marks and other facts, and have been led to conclude that a tolerably strong current swept round its shores; and that, in the centre, it was more tranquil and had a returning eddy. Comparing the directions of the currents with those of the contortions, I find, almost invariably, that the side on which the pushing force acted in the different parts of the ancient bay, agrees with that which would result from the drifting of an iceberg in the line of the current which prevailed there.

Since, as I have above described, whenever the contortions are well displayed, they are seen to be underlaid and surmounted by undisturbed beds, the time when they were formed is absolutely fixed within narrow limits; and, since they occur throughout the whole period of the drift, from the earliest to the most recent, it is clear that they cannot have been of contemporaneous formation. Hence we are compelled to suppose that the cause, whatever it might be, which produced them, acted during the deposition of every part. If, therefore, it is requisite to consider them to be due to the action of icebergs, we are led to the rather important theoretical conclusion, that they were present during the whole drift period, and not, as has been supposed by some highly distinguished geologists, merely at its close.

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